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The Revival of Byzantine Learning and the Revival of the Byzantine State

WARREN T. TREADGOLD

BETWEEN THE EIGHTH AND ELEVENTH CENTURIES the Byzantine Empire staged an almost unparalleled political and cultural revival, a recovery that is all the more striking because it followed a period of severe decline. By the early eighth century the empire had lost roughly two-thirds of the territory it had possessed in the year 600. Its remaining area was routinely raided by the Arabs and the Bulgarians, who at times threatened to take Constantinople and extinguish the empire altogether. The wealth of the state and its subjects was greatly diminished, and artistic and literary production had virtually ceased. By the early eleventh century, however, the empire had regained almost half of its lost possessions, its new frontiers were secure, and its influence extended far beyond its borders. The economy had recovered, the treasury was full, and art and scholarship had advanced to a high level. To be sure, the late eleventh century brought another serious decline, but, even so, the recovery of the preceding three hundred years was a remarkable achievement.[1]

To consider the political and cultural advances of this period as parts of a single phenomenon seems reasonable. After all, military, economic, and cultural progress have gone together in a number of other states and civilizations. Fifth-century Athens and Rome under Augustus provide the most obvious examples

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[1] There is no comprehensive study of the middle Byzantine period, as the seventh through eleventh centuries in Byzantium are commonly called, and many aspects of the period are not well covered in the secondary literature. Moreover, most of the sources are poorly edited, and a few, like the important *Chronicle* of Symeon the Logothete, remain unpublished. The best narrative, with a useful bibliography, is by George Ostrogorsky; see his *A History of the Byzantine State*, trans. Joan Hussey (2d ed., Oxford, 1968), chaps. 2–5. An inspired popularization, for which the documentation is, unfortunately, often not given and sometimes not extant, is Romilly Jenkins's *Byzantium: The Imperial Centuries, A.D. 610–1071* (New York, 1966). For an excellent treatment of the intellectual decline and subsequent revival, see Paul Lemerle, *Le premier humanisme byzantin: Notes et remarques sur enseignement et culture à Byzance des origines au X^e siècle* (Paris, 1971). And, for the most complete bibliographies for the period, see J. M. Hussey, ed., *The Cambridge Medieval History*, vol. 4: *The Byzantine Empire*, part 1—*Byzantium and Its Neighbors* (2d ed., Cambridge, 1966), 835–58. More specialized works are cited below.

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in antiquity. In Western Europe the formation of Charlemagne's empire corresponded to a "Carolingian Renaissance," the display of strength of the first four Crusades corresponded to a "Renaissance of the Twelfth Century," and the colonial expansion of the Age of Discovery accompanied the Renaissance itself. And, during modern times, the states representing Western European civilization gradually came to surpass the rest of the world not only in their military and economic power but also in their technology and cultural influence. In all such cases, examining the apparent connection between political and economic progress and cultural progress might help explain how both kinds of progress arose. Naturally, each case is different and requires separate treatment in detail.

Probably the easiest explanation of this apparent connection in the case of Byzantium would run like this: when the empire ended enemy raids on its own territory and began instead to raid and conquer enemy territory, Byzantine resources naturally expanded, and more money became available to patronize art and literature. Therefore, Byzantine military achievement led to economic advances, which in turn led to cultural revival. No doubt this hypothetical pattern did apply at times during the course of the recovery, particularly as greater wealth financed an artistic renaissance. Yet in other respects—at least if the findings of the last two generations of Byzantinists are correct—military advances do not seem to have come first, economic advances second, and intellectual advances third. If anything, the reverse seems to be true.

BYZANTINE MILITARY POWER HAD NEVER BEEN FULLY BROKEN. That the empire survived the attacks of the Arabs and Bulgarians between 650 and 850 was, in itself, a great accomplishment. But it was a defensive accomplishment, and, even as such, it was a limited success. The organization of the military provinces (themes), which covered not just the frontier districts but the whole territory of the empire, suggests that Byzantium was not trying so much to stop the raids as simply to hold on to as much land as possible after the raiders left. And at least Thrace and Asia Minor did remain Byzantine; but these territories were neither very safe nor pleasant places to live during this period. In the early ninth century the Bulgarians killed the emperor in a battle in Thrace and sacked its main towns. As late as 838 the Arabs sacked Ancyra (Ankara) and Amorium, the principal cities of Asia Minor, and in 843 or 844 they defeated the Byzantines at Mauropotamum, far inside imperial territory.[2] During the 850s the Byzantine army did better, but the real change occurred in the 860s. Henri Grégoire, George Ostrogorsky, and others have considered the campaign of 863 decisive in

[2] The Arabs killed Nicephorus I in 811 and sacked Sardica (Sofia) in 809, Develtus and Mesembria in 812, Hadrianople in 813, and Arcadiopolis in 814—all in Thrace; Steven Runciman, *A History of the First Bulgarian Empire* (London, 1930), 47–70. On Asia Minor, see A. A. Vasiliev and Henri Grégoire, *Byzance et les Arabes*, 1 (Brussels, 1935): 195–98; and, on the date of Mauropotamum, see Warren T. Treadgold, "The Chronological Accuracy of the *Chronicle* of Symeon the Logothete for the Years 813–845," *Dumbarton Oaks Papers*, 33 (forthcoming).

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freeing Asia Minor from Arab raids.[3] By 872, under Basil I, the Byzantines had conquered the Arabs' main allies on the border, the Paulician heretics, and permanently altered the military balance with the Abbasid Caliphate in the empire's favor. Thus, the beginning of the Byzantine military revival may be placed in the period between the 840s and the 870s.

The empire's economic position, however, had improved some years earlier. Though the government had never reached a state of bankruptcy in the seventh and eighth centuries, its expenses for defense and tribute were heavy and its resources shrinking. In the eighth century there was practically no building apart from some repairs.[4] Under Constantine V (741–75) Patriarch Nicephorus complained of extortionary taxation and a severe shortage of gold in circulation. Romilly Jenkins has argued that, when Empress Irene reduced taxes in 801, she provoked a fiscal crisis that led to her overthrow by her finance minister, who became Emperor Nicephorus I. Imperial finances were certainly in bad enough condition to cause Emperor Nicephorus to enact drastic financial reforms in 810, which the contemporary chronicler Theophanes denounced for their harshness.[5] After this, however, as J. B. Bury noted, no further signs of financial difficulty appear in the records until the eleventh century. Throughout the reign of Theophilus (829–42) the emperor carried on a program of building unparalleled since the sixth century.[6] Yet Theophilus left a large surplus in the treasury at his death, and fourteen years later his widow, Theodora, left an even larger one—amounting to about 60 percent of the record surplus left by Anastasius I in 518, when the empire was far larger and more secure.[7] Although her son, Michael III, spent most of the surplus, both the imperial finances and the economy continued strong during the subsequent reign of Basil I (867–86), when large-scale building resumed. The beginning of the empire's economic revival can, therefore, be placed between 810 and the 830s.

Finally, the Byzantine revival of learning appears to have begun even earlier. Although Byzantine education did not completely break down during the seventh and eighth centuries, Paul Lemerle has detected a marked cultural decline, reflected in the virtual cessation of copying manuscripts and a sharp decrease in

[3] See Ostrogorsky, *A History of the Byzantine State*, 227; Henri Grégoire in *The Cambridge Medieval History*, 4, pt. 1: 110–11; and Vasiliev, *Byzance et les Arabes*, 249–56.

[4] I know of only two major building projects in the eighth century: the repair of the walls of Constantinople in 740 and the repair of the Aqueduct of Valens in 767; Theophanes, *Chronographia*, ed. Carl de Boor (Leipzig, 1883), 412, 440. Justinian II had begun some new building in 694, financing it by large-scale exactions from the upper class; this heavy taxation contributed to his overthrow the following year; *ibid.*, 367–69.

[5] Jenkins, *Byzantium: The Imperial Centuries*, 103–04; and see below, pages 1262–64.

[6] J. B. Bury, *A History of the Eastern Roman Empire from the Fall of Irene to the Accession of Basil I (A.D. 802–867)* (London, 1912), 218–19. Bury plausibly estimated that Theophilus' buildings cost no less than the three hundred thousand pounds of gold spent by Justinian I on St. Sophia; *ibid.*, 221 n. 1. For the dates of the buildings, see Treadgold, "The Chronological Accuracy of the *Chronicle* of Symeon the Logothete."

[7] Anastasius' surplus was three hundred and twenty thousand pounds of gold; Procopius, *Historia Arcana*, ed. Jakob Haury (Leipzig, 1963), 19. 7. Theodora's surplus was one hundred and ninety thousand pounds of gold and three hundred pounds of silver; Theophanes Continuatus, *Chronographia*, ed. Immanuel Bekker (Bonn, 1838), 172; and Joseph Genesius, *Regum Libri Quattuor*, ed. A. Lesmüller-Werner and J. Thurn (Berlin, 1978), 4. 11. (I am skeptical about the editors' addition to line 91 of Genesius, because it disagrees with Theophanes Continuatus.)

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literary production, beginning as early as the late sixth century. During the sixty years of the first iconoclast period (728–88), the only important Greek writer, John of Damascus, lived in the caliphate, not in Byzantium. At about the same time as the restoration of the icons in 788, however, a number of notable scholars and writers began to appear. At least by 790, according to Jean Irigoin, the minuscule hand came into use. This invention, which made it possible to copy books much more quickly, cheaply, and legibly, has been compared in importance—with only some exaggeration—to the invention of printing. Judging from the quality of the literature of the early ninth century, Ihor Ševčenko has observed that the cultural revival was "in full bloom before the victory of

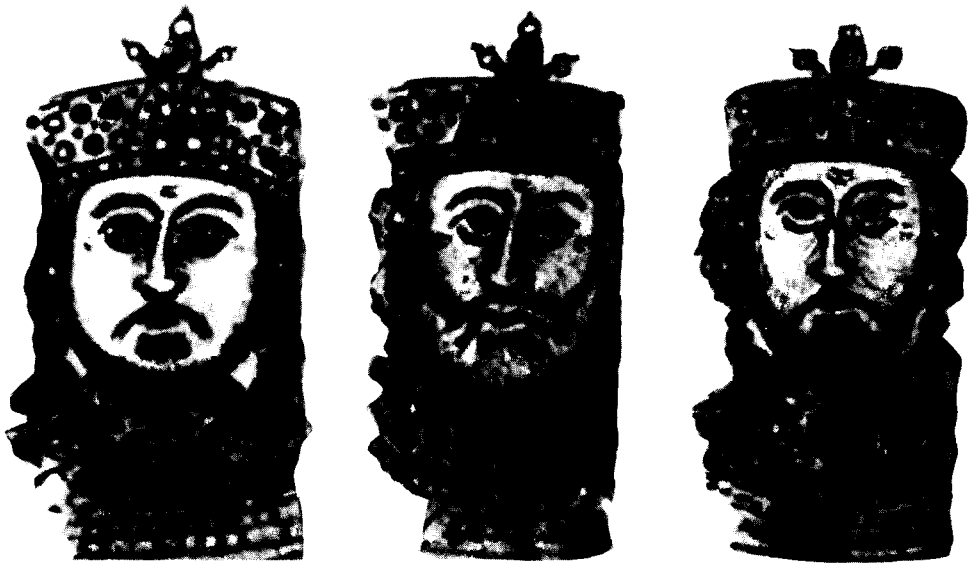


Figure 1a–1c: Miniatures of emperors of the Byzantine revival from a fifteenth-century manuscript of the *History* of John Zonaras (*Mutinensis Graecus* 112); the miniatures are probably derived from contemporary portraits. Photographs reproduced by courtesy of Iohannis Spatharakis, Leiden. *1a:* Constantine VI (780–97) became emperor at the age of ten but was overshadowed by his mother Irene (780–802). During their reigns the veneration of icons was restored (788) and iconophile scholars began the Byzantine intellectual revival. *1b:* Nicephorus I (802–11), who had been Irene's finance minister, instituted sweeping financial reforms (810) that in time contributed to the empire's economic revival. He died in battle against the Bulgarians. *1c:* Michael I (811–13) spent much of the gold reserve accumulated in the previous reign but apparently left his predecessor's reforms intact. Michael was severely defeated by the Bulgarians in 813.

[iconophile] Orthodoxy in 843."[8] From this time forward, in fact, the cultural level of the empire remained high; thereafter, no generation of Byzantines was without scholars of note until the fall of Constantinople in 1453. The beginning of the Byzantine revival of learning, then, can be placed in the late eighth century.

To recapitulate briefly, Byzantine scholarship revived under Constantive VI and Irene (780–802), a time when the Bulgarians and Arabs had the better of their battles with imperial troops and imperial finances were evidently in poor

[8] Lemerle, *Le premier humanisme byzantin*, 68–108, 121; Irigoin, "Survie et renouveau de la littérature antique à Constantinople (IX^{e} siècle)," *Cahiers de civilisation médiévale*, 5 (1962): 288; and Ševčenko, Review of *Le premier humanisme byzantin*, in *AHR*, 79 (1974): 1534.

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condition. The Byzantine economy revived in the period 810–30, when the imperial armies were soundly defeated by the Bulgarians and Arabs and imperial territory was constantly raided by the Bulgarians, Arabs, and rebel troops of Thomas the Slav.[9] Only in the middle of the ninth century, when the intellectual and economic recoveries were far advanced, did military recovery begin. Admittedly, the empire had not been in really desperate military or economic straits since the early eighth century, and, to be sure, modern knowledge of the Byzantine economy remains poor. It is difficult, however, to argue that the empire's intellectual revival did not begin well before its main military advances.

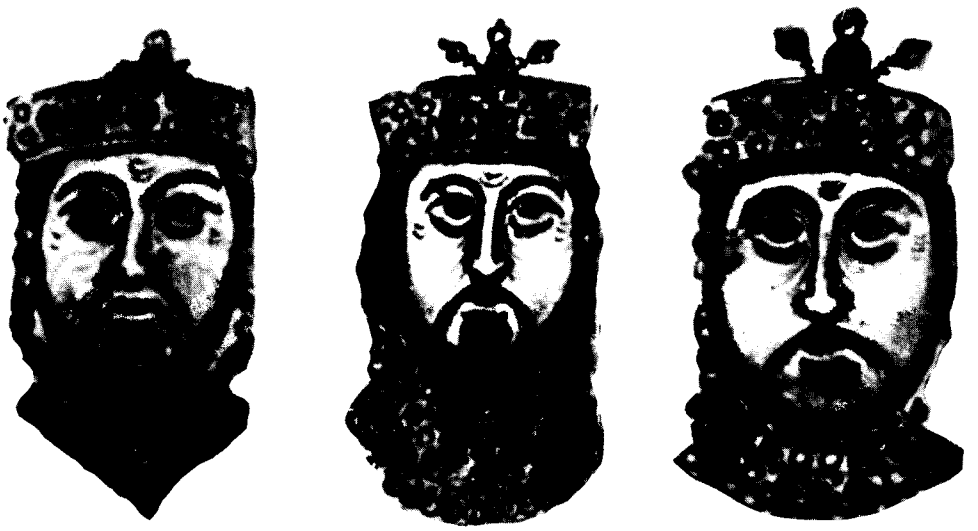


Figure 2a–2c: Miniatures of emperors of the Byzantine revival from a fifteenth-century manuscript of the *History* of John Zonaras. Photographs reproduced by courtesy of Iohannis Spatharakis, Leiden. *2a:* Michael II (820–29), like his predecessor Leo V (813–20), patronized iconoclast scholars. Michael even made one of them, John the Grammarian, tutor to his young son Theophilus. *2b:* Theophilus (829–42), whose reign was a time of intellectual brilliance and economic prosperity, founded the imperial school of the Magnaura (838), spent lavishly on buildings, and yet amassed a large gold reserve. He was, however, badly defeated by the Arabs (838). *2c:* Michael III (842–67) became emperor at the age of two, and until 856 his mother Theodora ruled. During his reign the Byzantines decisively defeated the Arabs (863) and began to convert the Slavs to Orthodox Christianity, though not until the reign of his successor Basil I (867–80) were Byzantine gains against the Arabs consolidated and made permanent.

That the intellectual revival came first does not, of course, prove that it was a cause of the economic and military recoveries. It was certainly not the only cause. Perhaps the most obvious reason for the empire's new military success was external: the abrupt decline of its greatest enemy, the Abbasid Caliphate, in the ninth century. An internal reason for the empire's economic and military recovery, emphasized in particular by George Ostrogorsky, was the rise of small landowners; invasions and resettlements had broken up many large estates, which made taxation and military recruitment easier for the Byzantine govern-

[9] On Thomas's rebellion, which between 821 and 823 reached most of both Asia Minor and Thrace, see Paul Lemerle, "Thomas le Slave," *Centre de recherche d'histoire et civilisation byzantines: Travaux et Mémoires*, 1 (1965): 254–97.

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ment. Peter Charanis and others have stressed the military and economic advantages of the new manpower provided by Armenians and Slavs who migrated into or were forcibly settled in the empire in the eighth and ninth centuries.[10] New manpower and better government control might well have led to economic progress before military progress, providing more money to pay for a bigger and better army.

Non-Byzantinists may also be ready to believe that advances in education and scholarship could have contributed to the empire's economic and military revival. But Byzantinists may be skeptical on two grounds. First, they know that the number of Byzantine scholars was always small and the circulation of their works limited. Lemerle has estimated that even in the tenth century those who received a full literary education at any one time were only two to three hundred.[11] If this estimate is correct, the total number of those with such an education was roughly three thousand. Second, Byzantinists know that Byzantine scholarship was a matter more of learning for learning's sake than of practical knowledge. A learned Byzantine was more likely to spend his time memorizing Homer than inventing gunpowder (though there *is* the well-known exception of Greek Fire). Indeed, many Byzantinists have claimed that a party of well-educated urban bureaucrats was largely responsible for ruining the empire in the late eleventh century by their persistent neglect of the army and the provinces.[12]

Nevertheless, impractical and pedantic though their scholarly efforts may have been, those few well-educated Byzantines were principally responsible for the administration of the church and state during the entire period of the recovery, and they cannot be denied some credit for it. Moreover, as Robert Browning has recently noted, the few who received a full literary education in the classics were only a small fraction of the educated. The empire's vast number of provincial officials and clergymen had to know how to read and write; inscriptions of ownership and other sources indicate that many civil and military officials in the provinces were not only literate but owned and read a number of books. Accounts in saints' lives show that an elementary education was available even in villages; signatures on documents reveal that most monks and many ordinary laymen were at least minimally literate. Browning has concluded that in Byzantium, unlike the situation in medieval Western Europe, "the ability to read and write did not mark out a man sharply from his fellows for life."[13] Most

[10] Marius Canard in *The Cambridge Medieval History*, 4, pt. 1: 711–27; Ostrogorsky, "La commune rurale byzantine," *Byzantion*, 32 (1962): 139–47; and Charanis, "Observations on the Demography of the Byzantine Empire," in *Thirteenth International Congress of Byzantine Studies: Main Papers*, 14 (Oxford, 1966): 1–19. Although Byzantinists have generally agreed that there was a rise of small landholders and that their increase benefited the state economically and militarily, Paul Lemerle has argued (persuasively, I think) against the existence of the "farmer-soldiers" that Ostrogorsky postulated for this period; see Lemerle, "Esquisse pour une histoire agraire de Byzance: Les sources et les problèmes," *Revue Historique*, 219 (1958): 32–74.

[11] Lemerle, *Le premier humanisme byzantine*, 257.

[12] See, for example, Speros Vryonis, "Byzantium: The Social Basis of Decline in the Eleventh Century," *Greek, Roman, and Byzantine Studies*, 2 (1959): 159–75.

[13] Browning, "Literacy in the Byzantine World," *Byzantine and Modern Greek Studies*, 4 (1978): 52–53, 39–54. In a lecture given at the Thirteenth Spring Symposium of Byzantine Studies at Birmingham in April 1979, Cyril Mango estimated that illiterate Byzantines were, "at a rough guess, 80 percent of the population." A literacy rate of 20 percent would be at least two million people. Though Mango later indicated that this rate is

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of Browning's evidence, however, dates from the ninth century and later. Here I propose to examine the beginnings of the revival of Byzantine learning and their possible effects on political developments.

THROUGHOUT BYZANTINE HISTORY THE SAME GROUPS both dominated the empire's intellectual life and took the main part in running the Byzantine church and state. This intelligentsia—to use that word freely—was composed of civil servants, church officials, and "urban" monks who lived in Constantinople and its environs. These men comprised the vast majority of those who had a secondary education, had read some classical literature, and could write fairly correct literary Greek and do advanced arithmetic without making too many mistakes. Although differing somewhat in outlook, these three groups differed little in education and in social status. Prominent clerics and monks came mostly from families of urban civil servants and had generally completed their education before they were ordained or took their vows. Ties of family and friendship and interlocking habits of officeholding bound these groups together. Civil servants were often promoted to high church office and some took monastic vows; many church officials were monks as well. Sometimes, certain outsiders entered one of these three groups by acquiring a literary education. It was this educated class, thus defined, who had to bring about an intellectual revival, if there was going to be one.

This class produced the empire's few leading writers of the early and middle seventh century: the historian Theophylact Simocatta, a civil servant under Heraclius (610–41); the poet George of Pisidia, a church official under Heraclius; and the theologian Maximus Confessor, a civil servant under Heraclius, who later became a monk. The empire's last major writer before the intellectual revival was the patriarch Germanus (715–30), a theologian, whose father had been a civil servant under Constans II (641–68).[14] But these times were not good for the Byzantine intelligentsia. Its schools were in decline; military crises kept it occupied; and the domestic upheavals between 695 and 717 resulted in the death, exile, or demotion of many of its members.

Throughout the eighth century this educated class does not seem to have had much enthusiasm for iconoclasm. Most urban monks, like other monks, were against it and suffered severely in the iconoclast persecutions.[15] Civil and eccle-

probably too high, one million literate people seems a quite conservative estimate for the tenth century. That figure provides a needed corrective to the estimate of three thousand with a complete classical education, which, as Browning has noted, is itself probably too low; "Literacy in the Byzantine World," 40.

[14] I have not counted John of Damascus, who was not a subject of the empire, among this group. On Theophylact, see Herbert Hunger, *Die hochsprachliche profane Literatur der Byzantiner*, 1 (Munich, 1978): 313–19; on George of Pisidia, see Gyula Moravcsik, *Byzantinoturcica*, 1 (2d ed., Berlin, 1958): 288–89; and, on Maximus and Germanus, see Hans-Georg Beck, *Kirche und theologische Literatur im byzantinischen Reich* (Munich, 1959), 436–42, 473–75.

[15] On the persecution of the monks, especially see Peter Brown, "A Dark-Age Crisis: Aspects of the Iconoclastic Controversy," *English Historical Review*, 88 (1973): 30–34. Brown's view that the monks were persecuted because they were iconophiles is the usual interpretation, which in my opinion has not been substantially undermined by a recent argument by Stephen Gero. Gero has admitted that Constantine V persecuted iconophiles, that the monks were iconophiles, and that a number of sources say that Constantine persecuted monks

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siastical officials were usually prevented from favoring icons openly because their careers were in the hands of iconoclast emperors. Nonetheless, when Leo III introduced iconoclasm in 726, men of rank and education were foremost among the protesters and were punished accordingly.[16] In 730, Patriarch Germanus was deposed for opposing iconoclasm.[17] The highest civil and ecclesiastical officials helped the usurper Artavasdus seize Constantinople in 741 and temporarily restore the icons.[18] In 766 nineteen high officials were punished for having conspired with the iconophile leader Saint Stephen the Younger; among this group may have been the father of the future patriarch Nicephorus, who was certainly punished for iconophilism under Constantine V.[19] In 780 Leo IV punished still another group of high officials for iconophilism, one of whom died and was counted as an iconophile martyr.[20] During the first period of iconoclasm, no Constantinopolitan official, cleric, or monk emerged as a leader of the iconoclast movement, although such men were the natural leaders of any theological movement. Instead, the educated class wrote little or nothing, leaving the defense of iconoclasm to the emperors and some provincial bishops from Asia Minor.[21]

In 780, however, the iconophile empress Irene took power and recalled the iconophile monks from exile. Almost at once, the civil servants, clerics, and monks in and around Constantinople began research, teaching, copying, and writing—in favor of icons. The incumbent patriarch repented of his toleration of iconoclasm and abdicated in 784; his successor, Tarasius, had been head of the civil service and was the most famous scholar of his day, an expert in classical poetry in particular. Under Tarasius, extensive research in the works of the

because they were iconophiles; but he still has speculated that the real reason Constantine persecuted monks was that the emperor was a libertine and hated ascetics; Gero, *Byzantine Iconoclasm during the Reign of Constantine V with Particular Attention to the Oriental Sources* (Louvain, 1977), 122–42. This conjecture, based on what look like iconophile slanders of Constantine's morality, seems to me to be revisionism for revisionism's sake.

[16] Theophanes stated—with obvious exaggeration—that these persecutions of the educated ended the Christian scholarly tradition that had prevailed in the empire since Constantine I; *Chronographia*, 405. 5–14.

[17] Germanus retired to his ancestral estate; Theophanes, *Chronographia*, 408. 31–409. 11; and Nicephorus, *Breviarium*, ed. Carl de Boor (Leipzig, 1880), 58. 17–24.

[18] The *magister militum in praesenti* Theophanes Monotes, whom Constantine V had left in charge of the capital, and the patriarch Anastasius both sided with Artavasdus; Theophanes, *Chronographia*, 415. 2–22; and Nicephorus, *Breviarium*, 60. 14–24. Although the iconophilism of Anastasius and even of Artavasdus may have been opportunistic, even opportunistic iconophilism on their part would be a sign of genuine iconophile sentiment among the ruling class in Constantinople.

[19] The leader of the conspiracy was Constantine Podopagurus, Logothete of the Drome, the principal civil official; Theophanes, *Chronographia*, 438. 2–26. (In line 7, *τὸν προορηθέντα ἔγκλειστον* is Stephen; *ibid.* Also see *ibid.*, 436. 28–29.) Patriarch Constantine was also charged with complicity, but Theophanes implied that the charge was false; *ibid.*, 438. 26–439.5. On Nicephorus' father, see Paul Alexander, *The Patriarch Nicephorus of Constantinople* (Oxford, 1958), 56.

[20] Theophanes, *Chronographia*, 453. 10–20. As every interpretation of the eighth century must, my interpretation here relies heavily on the chronicles of Theophanes and Nicephorus, which show a definite pro-iconophile bias; see page 1251, below. But for the present purposes they seem trustworthy: a pro-iconophile bias is not a reason for honoring as iconophiles disaffected officials and monks who were not really iconophile. Of course, their iconophilism does not mean that these men may not also have had other reasons for their disaffection with the iconoclast emperors.

[21] Stephen Gero, in his two monographs on the first iconoclastic period, has convincingly argued that the emperors Leo III and Constantine V were their own most important theologians, though certain provincial bishops did play a part in defending iconoclasm; Gero, *Byzantine Iconoclasm during the Reign of Leo III with Particular Attention to the Oriental Sources* (Louvain, 1973), esp. chaps. 8–11, and *Byzantine Iconoclasm during the Reign of Constantine V*, *passim*.

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Church Fathers was done to support the restoration of the icons at the Second Council of Nicaea in 788. Tarasius' successor as patriarch was Nicephorus (806–15), who had earlier been both a civil servant and a monk and was himself a noted scholar.[22] Nicephorus wrote theological works against the iconoclasts and a chronicle with an anti-iconoclast slant.

Tarasius appointed as his patriarchal secretary the monk George Syncellus. George set to work on an ambitious world chronicle that was completed after his death in 810 or 811 by his friend Theophanes, a monk from a family of civil servants. Theophanes' continuation, completed according to George's plan, is strongly anti-iconoclast.[23] In 806, the Constantinopolitan deacon Stephen wrote the notable *Life of Saint Stephen the Younger*, whose hero was the iconophile leader martyred under Constantine V.[24] Under the patriarchates of Tarasius and Nicephorus and even earlier, Abbot Plato of the Bithynian monastery of Saccudium, another former civil servant, was perfecting the minuscule hand, in which he copied many theological works. Plato specialized in florilegia from the Fathers, which doubtless gave prominence to texts in support of the icons. At the same time Plato's cousin, Abbot Theodore of Studium, the well-educated son of a civil servant, wrote voluminous and elegant theology, poetry, and epistolography. Under him, the monastery known as the Studium in Constantinople became a center both of learning and of opposition to iconoclasm.[25]

Therefore, it seems fair to say that the educated class in Byzantium, which was also the traditional ruling class, disliked iconoclasm and opposed it when the opportunity arose. This conclusion would agree with the impression of Hélène Ahrweiler about iconoclasm—that "the common people followed official decisions, especially in Constantinople, whereas the traditional ruling classes remained attached to the old traditions."[26] No doubt there were exceptions, but the groups that had administered church and state in the past would naturally have resented a repudiation of past practices. Whatever its origins, iconoclasm effectively centered power in the hands of the emperor at the expense of the church and the monks, and this transfer of power the educated class resented as well.[27] The challenge that this doctrine presented roused that intelligentsia from a prolonged period of intellectual inactivity into a renewal of copying, researching, and writing. A group of educated Byzantines capable of staging an in-

[22] On the research done for this council, see Cyril Mango, "The Availability of Books in the Byzantine Empire, A.D. 750–850," in *Byzantine Books and Bookmen: A Dumbarton Oaks Colloquium* (Washington, 1975), 29–35. On both Tarasius and Nicephorus, see Lemerle, *Le premier humanisme byzantin*, 128–35.

[23] On George Syncellus and Theophanes, see Hunger, *Die hochsprachliche profane Literatur der Byzantiner*, 331–32, 334–39. Cyril Mango has recently argued that the *Chronographia* attributed to Theophanes is almost entirely the work of George; Mango, "Who Wrote the Chronicle of Theophanes?" *Zbornik radova Vizantinološkog instituta*, 18 (1978): 9–17. Mango's argument is very persuasive in the main, but I doubt his attribution to George of the *Chronographia*'s prejudice against Nicephorus I. At the very least, Theophanes allowed the violent criticism of Nicephorus to remain in the text.

[24] See Ihor Ševčenko, "Hagiography of the Iconoclast Period," in Anthony Bryer and Judith Herrin, eds., *Iconoclasm* (Birmingham, 1977), 115–16.

[25] Lemerle, *Le premier humanisme byzantin*, 122–28; and Theodore of Studium, *Laudatio Platonis*, in *Patrologia Graeca*, 99: col. 820A.

[26] Ahrweiler, "The Geography of the Iconoclast World," in Bryer and Herrin, *Iconoclasm*, 27.

[27] See Gerhart Ladner, "Origin and Significance of the Byzantine Iconoclastic Controversy," *Mediaeval Studies*, 2 (1940) 127–49; and Brown, "A Dark-Age Crisis: Aspects of the Iconoclastic Controversy," 1–34.

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tellectual revival had always existed; what was needed was the impetus provided by the threat of iconoclasm.[28]

This impetus, rather than any increase in wealth or patronage, appears to explain the revival of learning. Though an iconoclast, Emperor Constantine V was an educated man and sponsored much theological research in the iconoclast cause.[29] But he failed to attract scholars of real distinction to his side, a sign of the unwillingness of the best-educated Byzantines to join him. Conversely, the iconophile empress Irene, although she sponsored the Second Council of Nicaea, was not a founder of schools or libraries or a patroness of learning in the strict sense. To be sure, she appointed learned men to high office, but no more than most Byzantine rulers did; the bureaucratic careers of Tarasius and Nicephorus had already begun under the iconoclast emperors. All of the major scholars of the first generation of the revival of learning pursued scholarship in their spare time: by profession they were civil servants, church officials, and monks, not teachers or researchers. Their scholarly activities cost the state little or nothing that would not have been paid out anyway. Although some apparently advanced their careers through their scholarship, their desire to advance the iconophile cause seems beyond dispute.

During the second iconoclast period (815–43), the emperors evidently realized how helpful scholars had been in restoring the icons and made deliberate efforts to recruit scholars to help in banning the icons again. Although the emperors do not seem to have had much success in winning educated men of the old families to iconoclasm, imperial patronage was more attractive to lower-class scholars who had recently entered the ranks of the educated—a category that the revival of learning had naturally expanded. Not surprisingly, some who had been won to iconoclasm through patronage changed sides as soon as the icons were restored.

The most enthusiastic and influential of the new iconoclast scholars was John the Grammarian, whom Leo V appointed to head the theological commission that prepared for the reintroduction of iconoclasm. Leo would have made John the patriarch in 815 if "the patricians" had not objected that John was "young and obscure."[30] Michael II made John the tutor to his son Theophilus; as emperor, Theophilus treated John as a trusted adviser and finally did make him patriarch in 838. Later the same year, Theophilus created a professorship for John's nephew, the learned Leo the Mathematician, who had reportedly been poor and unknown in earlier years. In 840 Theophilus honored Leo by making him the metropolitan of Thessalonica. Another important iconoclast scholar was Ignatius the Deacon, an epistolographer, hagiographer, and poet, who had

[28] Compare this assessment with the observation of Cyril Mango on iconoclasm and book production: "While in itself Iconoclasm had no connection with book production, let alone a classical revival, it certainly gave an impulse to a more systematic collection and study of Patristic literature. Once put in motion, this current eventually spilled over into secular literature." "The Availability of Books in the Byzantine Empire," 44–45.

[29] Mango, "The Availability of Books in the Byzantine Empire," 29–31.

[30] Immanuel Bekker, ed., *Scriptor incertus de Leone Armenio*, appendix to Leo Grammaticus, *Chronographia* (Bonn, 1842), 359. 16–20.

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been a student of the Patriarch Tarasius but joined the iconoclasts while they were in power. Theophilus appointed Ignatius metropolitan of Nicaea.[31] Ignatius and Leo, but not John, joined the iconophiles when they finally emerged victorious in 843.

Even during the second period of iconoclasm, iconophile scholars of the traditional kind were still active. Theodore of Studium and Patriarch Nicephorus continued to write under Leo V and Michael II. Theophilus treated iconophile scholars with a mixture of hostility and respect. He kept Saint Methodius, an abbot and polymath, under arrest at court in order to benefit from his learning.[32] The emperor was intimidated by the noble poetess Cassia when she was a candidate for his hand; rejected, she became an abbess and hymnographer.[33] Theophilus condemned the bureaucrat-historian Sergius Confessor for iconophilism and exiled him with his wife and children; one of Sergius' children appears to have been Photius, perhaps the best of all Byzantine scholars. During this exile Photius probably did much of the reading and notetaking for his famous literary compendium known as the *Bibliotheca*.[34] Photius' career belongs mostly to a period, the so-called Macedonian Renaissance, that modern scholars have long recognized as a period of scholarly revival.[35]

Such were the scholars of the first rank in the early ninth century. About the second rank naturally much less is known. Several of the famous scholars had students, sometimes many. Tarasius taught Ignatius the Deacon and possibly others. Theodore of Studium, too, had students, and other monks at the Studite school taught extensively. The teacher of Leo the Mathematician, whose name is not known, is described as a learned man; he presumably instructed others. Leo, Ignatius, and John the Grammarian all taught, and Leo's students were many.[36] According to one Byzantine source, an unnamed student of Leo's who was captured by the Arabs in 838 could provide explanations and demonstrations of Euclid that the court geometers of the caliph could not match. When the caliph asked the student how many men in Constantinople were as learned

[31] On the dates of John's patriarchate and Leo's professorship, see Treadgold, "The Chronological Accuracy of the *Chronicle* of Symeon the Logothete." On John and Leo more generally, see Lemerle, *Le premier humanisme byzantin*, 135–47, 148–76. (Lemerle also mentioned another low-born scholar who joined the iconoclasts, Antonius Cassimates; he was made the patriarch of Constantinople in 821 by Michael II; *ibid.*, 140–41.) On Ignatius, see Cyril Mango, "Observations on the Correspondence of Ignatius, Metropolitan of Nicaea (First Half of the Ninth Century)," *Festschrift Marcel Richard* (forthcoming).

[32] Theophanes Continuatus, *Chronographia*, 116.

[33] On the authenticity of Cassia's encounter with Theophilus, see Treadgold, "The Problem of the Marriage of the Emperor Theophilus," *Greek, Roman, and Byzantine Studies*, 16 (1975): 325–41. Otherwise on Cassia, see Ilse Rochow, *Studien zu der Person, den Werken, und dem Nachleben der Dichterin Kassia* (Berlin, 1967).

[34] On Photius, see Cyril Mango, "The Liquidation of Iconoclasm and the Patriarch Photius," in Bryer and Herrin, *Iconoclasm*, 135–39. The *Bibliotheca* was probably composed in 845 and represented work done over many years before that; see Warren T. Treadgold, *The Nature of the* Bibliotheca *of Photius*, Dumbarton Oaks Studies, no. 18 (forthcoming).

[35] On the Macedonian Renaissance, see Lemerle, *Le premier humanisme byzantin*, chaps. 7–10, and the references cited therein.

[36] See Theodore's letters to his students in *Patrologia Graeca*, 99: cols. 937C–940B, 940C–944B, 1373B–1376D, 1404B–1405C, 1421B–1425A, 1460B–1461C; and Ignatius' letter to his student Constantine in Ignatius, *Epistolai*, ed. Manuel Gedeon (Constantinople, 1903), no. 57, cols. 54–55. Also see Mango, "The Availability of Books in the Byzantine Empire," 29–35; Lemerle, *Le premier humanisme byzantin*, 135–47, 148–76; Theodore of Studium, *Laudatio Platonis*, in *Patrologia Graeca*, 99: col. 820A; and Bekker, *Scriptor incertus de Leone Armenio*, 359: 16–20.

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Figure 3a–3b: Two miniatures illustrating the story of Leo the Mathematician from a thirteenth-century manuscript of the *Chronicle* of John Scylitzes (*Martritensis*, II). Photographs reproduced by permission of the Biblioteca Nacional, Madrid. *3a (top):* At left, at the caliph's court, three Arab geometers marvel at the geometrical proofs just given by a Byzantine prisoner of war who had studied under Leo the Mathematician. At right, in Constantinople, three messengers deliver a letter from the caliph to Leo, inviting him to come to the Arab court to teach. *3b (bottom):* At right, the caliph, disappointed that Leo has declined his offer and accepted a teaching position from the Byzantine emperor Theophilus, sends an ambassador to the emperor with a letter. At center, the ambassador journeys to Constantinople. At left, Theophilus receives the letter, which offers him a peace treaty in return for a brief visit by Leo to the caliphate.

as he, the student replied that they were numerous and ranked himself among the students rather than the masters.[37] Some seven years later Photius wrote to his brother, "In our day, in geometry, arithmetic, and the other sciences, as you know as well as I do, there are many among our acquaintances who have no less exact knowledge, I dare say, than [Ammonius] the son of Hermias . . ."—a typically Byzantine way of saying that Byzantine scholars were as good as those of antiquity. But in the same passage Photius complained about the practice of

[37] Theophanes Continuatus, *Chronographia*, 186–87. On the date, see Treadgold, "The Chronological Accuracy of the *Chronicle* of Symeon the Logothete."

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discarding books that was common in earlier times, making it plain that the enlightenment of his day represented a major change.[38]

The invention of the minuscule hand in particular made possible an expansion of Byzantine education and literacy outside the urban educated class and outside the capital. Even after the use of minuscule had become widespread, books were still expensive and scarce, but they were considerably cheaper and far more plentiful than they had been before.[39] No doubt the number of literate and well-educated Byzantines, while still small, increased substantially after the first restoration of the icons in 788. Ann Moffat's recent survey of education in the iconoclastic period, based on hagiography, seems to show evidence of this increase. Of the eleven saints whom she principally studied, four were born during and seven after the first period of iconoclasm. Of the four saints born before 788, two were uneducated and two were urban monks educated in or near Constantinople. Of the seven born later, all were literate, including six educated in the provinces. Two were born just after 788: Theodore of Edessa (793–after 860), whose schooling in Edessa as described in his *Life* inspires some doubts, and Nicholas the Studite (793–868), who was probably illiterate when he came from Crete as a youth to be educated at the Studite school in Constantinople.[40] Although small, Moffat's sample suggests the spread of schooling that might be expected.

Much work remains to be done on the letters of Theodore of Studium, but the list of his correspondents demonstrates just how widespread education was in this period. Because Theodore's letters are always eloquent and usually erudite, these correspondents must have been people of some education. Theodore wrote at least eleven hundred and twenty-four letters, of which less than half (five hundred and sixty-one) survive.[41] While it is sometimes hard to tell which correspondents of the same name are the same people, our collection appears to have been written to roughly three hundred and thirty different addressees, including thirty-one groups. The total includes some fifty civil servants, thirty officials of the secular church, fifty abbots and abbesses, and at least seventy-five

[38] On this passage, see Warren T. Treadgold, "Photius on the Transmission of Texts (*Bibliotheca*, Codex 187)," *Greek, Roman, and Byzantine Studies*, 19 (1978): 171–75.

[39] See Mango, "The Availability of Books in the Byzantine Empire," 29–35. Mango has, I believe, overstated the rarity of books in the ninth century. His estimates of the number of books used by George Syncellus and Theophanes are exceedingly low, and he has minimized the importance of the private library of Photius (I have calculated that it contained, at a minimum, one hundred and fifty volumes); see my *The Nature of the* Bibliotheca *of Photius*, chap. 6. Nigel Wilson has arrived at low estimates of several Byzantine libraries by counting only books from them that we know of today—as if we can know of more than a small fraction of the books that then existed. The longest list, the seventy-eight volumes in the will of Eustathius Boilas, is the only source that is likely to be complete. See Wilson, "Books and Readers in Byzantium," in *Byzantine Books and Bookmen*, 7–8.

[40] The uneducated were Saints Joannicius (746–846) and John of Psicha (d. 820/29); the educated urban monks born before 788 were Saints Stephen the Younger (713–65) and Nicephorus of Medicium (755–813). Those born after 788 were Saints Evaristus the Studite (b. 819), educated in Galatia; Joseph the Hymnographer (816–86), educated in Sicily; Nicholas the Studite; Constantine the Jew (probably born late in the second iconoclastic period), educated in Phrygia; Theodore of Edessa; and Theodora of Thessalonica (born early in the ninth century) and Athanasia of Aegina (ninth century), both educated in Aegina. Ann Moffat, "Schooling in the Iconoclast Centuries," in Bryer and Herrin, *Iconoclasm*, 86–90.

[41] See Bury, *A History of the Eastern Roman Empire*, 451–52; and Patrick O'Connell, "The Letters and *Catecheses* of St. Theodore Studites," *Orientalia Christiana Periodica*, 38 (1972): 256–57.

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ordinary monks and nuns. Among those who might be expected to be uneducated are at least fifty identifiable provincials (and probably far more who are not identifiable), about fifty women (of whom approximately half were wives of civil servants and half were nuns), many men of unspecified status, and some of low status, including a perfume-seller and a chandler.[42] Of ninety-one letters that have been dated, only five date before 800, twenty-three between 800 and 809, forty-six between 810 and 819, and seventeen between 820 and Theodore's death in 826.[43] The dates of the letters themselves may indicate that education was spreading at this time, especially because the later addressees are noticeably lower in rank than the earlier ones.[44]

OF WHAT POLITICAL BENEFIT WAS the Byzantine revival of learning? Almost all of the famous scholars of the period of revival held positions of responsibility in the civil service of the capital, in the church of Constantinople (which was, in a sense, a branch of the civil service), or in urban monasteries that had considerable political influence. What did they accomplish for the empire?

One of the emperors' principal objectives was to persuade Byzantines that official ideas of Orthodoxy were correct. In 788, after fifty years of iconoclasm, the scholarly Tarasius succeeded in restoring the icons for Irene. And the scholarly John the Grammarian was instrumental in banning them again in 815 for Leo V. In 843, Empress Theodora chose the learned Saint Methodius to restore the icons for good. If Orthodoxy was a benefit—and there are some purely secular reasons for considering iconoclasm a cultural step backward—Byzantine scholarship deserves much of the credit for ending iconoclasm.[45] The re-establishment of Orthodoxy brought improved relations with Christians in the West and in the caliphate, virtually none of whom were iconoclasts.

Byzantine scholarship also contributed to the empire's diplomacy. Emperor Theophilus sent John the Grammarian on an embassy to the Arabs, and Photius was part of an embassy to the Arabs in the reign of Michael III. Theophilus showed off the empire's technology by receiving foreign ambassadors in a room equipped with mechanical lions that roared and mechanical birds that sang. When Theophilus learned that the caliph had invited Leo the Mathematician to teach at the Abbasid court, the emperor appointed Leo to teach in a school in his own palace. During negotiations after the Byzantine defeat at Amorium in 838, the caliph allegedly offered better terms in return for a visit by

[42] I have made my calculations from the index of correspondents in *Patrologia Graeca*, 99: cols. 1855–58, checking the texts for clues to the status of correspondents when it is not specified, and from the list of other letters in *ibid.*, cols. 1669–80.

[43] See the chronological index in *Patrologia Graeca*, 99: cols. 1857–58.

[44] Of course, Theodore's connections and correspondence would have tended to expand as he grew older, but I do not believe that this expansion explains the whole difference. Since Theodore was born in 759, he was already forty-one in 800. The basic character of the letters excludes other objections: that some of Theodore's letters are not real letters, that they have been extensively rewritten, or that some of his correspondents had his letters read to them by others. I do not think that anyone who has studied the letters would doubt that they are real letters without extensive retouching or that the recipients were educated people who were usually writing to Theodore themselves.

[45] See Lemerle, *Le premier humanisme byzantin*, 107–08.

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Leo. Theophilus refused, on the ground that it would be folly to hand over the Byzantines' knowledge to foreigners.[46] Scholarship was in fact an important part of the empire's carefully nurtured prestige, which under Theophilus took on a brilliance that became legendary despite his military failures. Nor was the prestige of Byzantine scholarship all empty show: Leo the Mathematician designed for Theophilus an optical telegraph linking the Arab frontier with Constantinople that had both diplomatic and military value.[47]

Probably the greatest triumph of Byzantine diplomacy was the extension of Byzantine influence over the Slavs through their conversion to Eastern Orthodoxy; for this, Byzantine scholarship was instrumental. The close connection of scholars with the missionary effort suggests that the Slavs accepted Orthodox Christianity largely in deference to the Byzantines' superiority in learning. The conversions began with the mission to Moravia of Saint Constantine-Cyril. Constantine-Cyril learned the Slavonic language and invented a Slavonic alphabet, which made possible the subsequent translation of the Bible and other works from Greek into Slavonic—an achievement directly attributable to the intellectual revival, because Constantine-Cyril was a student of both Leo the Mathematician and Photius.[48] As patriarch of Constantinople, Photius was himself the central figure in converting the Bulgarians and aligning them with the Byzantine Church as well as in sending the first mission to the Russians.[49]

The activity of the great scholars, however, was probably less important in beginning the political recovery than was the gradual rise in the number of educated men and in the level of their education. Ramsay MacMullen has recently argued that an important cause of the political decline of the Roman Empire in late antiquity and the early Middle Ages was the decline in the ability of the civil service to express itself clearly in writing, to keep usable records, and to handle numbers.[50] Conversely, an important cause of the political revival of the same empire in the ninth century (and of course it *was* the same empire) was the increasing ability of the bureaucracy to perform these tasks.

That the civil service had always been literate and able to do arithmetic was only natural in a state as centralized as Byzantium was. The tax system required a list of all of the heads of households and an assessment of all the cultivated land, classified by use and quality. These records were used every year to collect

[46] On John's embassy, datable to 830, see the references in Treadgold, "The Chronological Accuracy of the *Chronicle* of Symeon the Logothete"; and, on Photius' embassy, probably datable to 845, see Treadgold, *The Nature of the* Bibliotheca *of Photius*, chap. 2. For references concerning the mechanical devices, which were probably put in place between 834 and 836, see Treadgold, "The Chronological Accuracy of the *Chronicle* of Symeon the Logothete." And, on Leo's court appointments, see, for the date, *ibid.*; and, for the narrative, Theophanes Continuatus, *Chronographia*, 189–90.

[47] See Charles Diehl, "La légende de l'empereur Theophile," *Seminarium Kondakovianum*, 4 (1931): 33–37; and Lemerle, *Le premier humanisme byzantin*, 154–55.

[48] On Byzantine influence among the Slavs in general, see Dimitri Obolensky, *The Byzantine Commonwealth: Eastern Europe, 500–1453* (London, 1971); and, on the mission to the Moravians, see *ibid.*, 137–53. On Constantine-Cyril's mentors, see Ševčenko, Review of Lemerle's *Le premier humanisme byzantin*, 1533. Lemerle has, I believe incorrectly, remained unconvinced that Constantine-Cyril was ever a pupil of Leo or of Photius; for his argument, see Lemerle, *Le premier humanisme byzantin*, 163–65.

[49] On the mission to the Bulgarians, see Obolensky, *The Byzantine Commonwealth*, 84–97; and, on the mission to the Russians, see *ibid.*, 183–84.

[50] MacMullen, *The Roman Government's Response to Crisis: A.D. 235–337* (New Haven, 1976), 48–70.

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the land tax, hearth tax, yoke tax, and pasture tax as well as four proportional surcharges. Most imports had to be valued and subjected to customs duties according to a schedule.[51] Numerous, complicated laws had to be enforced. The army was stationed all over the empire, in places and numbers recorded on muster rolls. The soldiers' pay, sent out to them periodically from Constantinople, was dispensed according to payrolls. For every general offensive or defensive campaign, the soldiers had to be assembled from their usual stations at specified staging areas. At times the emperors ordered thousands of their subjects to be resettled in other parts of the empire, an order of great complexity. Since the state did not borrow or debase its coinage in this period, the budget had to balance every year or the deficit had to be made up from reserves that had been saved in previous years by careful management. The church, a virtual department of the state with many philanthropic functions, also had a far-flung administration and a budget to balance. These tasks were always performed, but, of course, they were not always performed equally well.

For them to be done well, many bureaucrats were needed to keep and consult correctly a very large number of handwritten records and accounts. As all who have studied manuscripts know, even the best scribes make copying errors, particularly in copying numbers; and the worst scribes make a great many. Laws and orders were written in literary (Koine) Greek, even though by this time spoken Greek was very different in vocabulary, grammar, and syntax—and in pronunciation, which affected spelling. Learning to read and write thus involved many of the problems of learning a foreign language. Even the well educated had problems. In the middle of a tenth-century manual of regulations for assessing land, someone inserted a palindrome verse that may be translated, "O law, my brain could grasp running water more easily than it could grasp you."[52] For anyone who has waded through the manual's bureaucratese, the verse is no joke. By being unable to understand such rules, many officials must have made assessments that were much too high or too low, with serious consequences for the taxpayers and the state.[53]

There was, in addition, the problem of doing arithmetic without Arabic numerals or a zero. The Byzantines had inherited one of the least valuable legacies of the ancient Greeks—their system of numerical notation. In Greek notation, the accented letters of the Greek alphabet stood, in order, for the numbers from one to nine, next for those from ten to ninety in increments of ten, and then for those from one hundred to nine hundred in increments of one hundred. The let-

[51] For the standard work on imperial finances, still see Franz Dölger, *Beiträge zur Geschichte der byzantinischen Finanzverwaltung* (Leipzig, 1927; reprint ed., with addenda and corrigenda, Hildesheim, 1960). Also see George Ostrogorsky, *Die ländliche Steuergemeinde des byzantinischen Reiches im X. Jahrhundert* (Stuttgart, 1927; reprint ed., with postscript, Amsterdam, 1969).

[52] *Fiscal Treatise*, 113. 23, edited in Dölger, *Beiträge zur Geschichte der byzantinischen Finanzverwaltung*, 113. 23: "Ἐμὸν νόα ῥοῦς ἕλε σοῦ ῥᾶον, νόμε." (Literally, "Running water has seized my mind more easily than you, O' law.") For a description and translation of this treatise and another like it, see Charles M. Brand, "Two Byzantine Treatises on Taxation," *Traditio*, 25 (1969): 35–60. (But Brand omits the verse from his translation.)

[53] For example, the author of the manual noted that some assessors made the mistake of taking the total amount of tax with the four surcharges included and then adding the surcharges to the tax again; *Fiscal Treatise*, 122. 34–123. 1; and Brand, "Two Byzantine Treatises on Taxation," 56–57. This would have increased the total owed by about 23 percent over the correct amount.

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ters were re-used with accent marks beneath them to reach nine hundred thousand. Any of these letters written above an "M" multiplied the value by ten thousand. Fractions were written by using double accents and superscripts. Thus, the possibilities for misreading numbers were endless. Addition and subtraction under such a system, to say nothing of multiplication and division or the problems involved with fractions, were far more complicated than they are in Arabic notation.[54] But multiplication, division, and fractions were essential for assessing taxes and figuring budgets.

The poverty of the sources for the seventh and eighth centuries precludes any clear account of the effects of the decline of education on imperial administration. The seventh century did see the emergence of the system of military provinces (themes), under which the administrative levels in the empire were reduced from four to two. The new system consisted of the central government and five themes, ruling territory that earlier had been administered by the central government and the descending bureaucratic tiers of two pretorian prefectures, five dioceses, and twenty-six civil provinces. This change decreased the number of civil servants in the provinces and, consequently, eased the problem of recruiting them; it also concentrated the empire's system of taxation and allocation in the hands of the central bureaucracy.[55] But how well did this system function?

The chroniclers Theophanes and Nicephorus reported that the iconoclast emperors of the eighth century managed to pile up a substantial surplus of gold by minimizing expenditures, confiscating the property of iconophile monasteries, and raising the taxes of both farmers and city dwellers. In 767, according to Nicephorus, this situation led to a gold panic, in which landowners had to sell their crops at a fifth of the normal price in order to obtain enough gold to pay their taxes.[56] In a letter to Empress Irene, Theodore of Studium deplored the ruinous taxes to which the working class was subjected under her predecessors, when tax collectors were everywhere. They were said to have demanded from fishermen the price of one out of every three fish. Both Theodore and Theophanes praised Irene for reducing city taxes and trade duties.[57] Despite these reductions, Irene still had some gold reserve when she was overthrown in 802.[58]

[54] We must memorize 180 arithmetical combinations (if we are to do without our pocket calculators), but the Byzantines, using Greek notation, had to memorize 1,026. See Nicomachus of Gerasa, *Introduction to Arithmetic*, trans. Martin D'Ooge (New York, 1926), 66–70. In practice the Byzantines often used shortcuts (the abacus for one), but the shortcuts themselves undoubtedly led to serious errors in poorly trained hands.

[55] For a clear and recent survey of these administrative changes, see M. F. Hendy, "On the Administrative Basis of the Byzantine Coinage, c. 400–c. 900, and the Reforms of Heraclius," *University of Birmingham Historical Journal*, 12 (1970): 129–37. In counting provinces and themes, I have followed J. Karayannopulos, *Die Entstehung der byzantinischen Themenordnung* (Munich, 1959), maps 1, 3, whose findings seem generally reliable.

[56] For tax increases under Leo III, see Theophanes, *Chronographia*, 410, 412; and, for those under Constantine V, see Nicephorus, *Antirrheticus*, in *Patrologia Graeca*, 100: cols. 513C–516A. For confiscations under Constantine V, see Theophanes, *Chronographia*, 443, 445–46. And, for the gold panic, see Nicephorus, *Breviarium*, 76. Nicephorus reported that in 767 wheat sold at sixty modii per nomisma; *ibid.* The normal price was about twelve modii per nomisma; George Ostrogorsky, "Löhne und Preise in Byzanz," *Byzantinische Zeitschrift*, 32 (1932): 321–22. Nicephorus' report does not, of course, mean that the gold in circulation fell to only 20 percent of the normal amount; the psychology of panic, which would have particularly affected the area around Constantinople when taxes fell due, would have greatly aggravated the problem.

[57] Theodore, *Patrologia Graeca*, 99: cols. 929B–933C. Also see Theophanes, *Chronographia*, 475.

[58] Theophanes, *Chronographia*, 476.

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These scattered complaints of fiscal oppression and reports of surpluses prove little about the efficiency of the administration. The tax system did produce substantial revenue, especially in combination with wholesale confiscation of monastic properties. But the tax rates were evidently high, so that even an inefficient system could have been lucrative. Theodore, who gave the most detailed description, wrote of widespread corruption among the tax collectors and their demands from the poor for more than was due.[59]

The first really useful evidence for the financial administration of the middle Byzantine state is Theophanes' lengthy description of the financial measures of Emperor Nicephorus for 810 and 811. Theophanes wrote only two or three years after the events he described. Though he declared that Nicephorus' financial measures were intolerably oppressive, most modern scholars have seen them as reforms of some merit. Nicephorus, who had been Irene's finance minister, was in a position to know what reforms were needed and how to enact them. Nicephorus' plans allow us to infer the abuses he wished to correct.

According to Theophanes, Nicephorus ordered a new census of the whole empire, probably the first in at least seventy-five years and badly needed. Aside from a modest roll tax collected at the time of the census, the tax rates appear not to have been raised.[60] The census began perhaps in late 807 and was completed in 810.[61] Apparently, it turned up evidence of massive and long-standing tax evasion by the wealthy. Theophanes complained that the emperor's inspectors went everywhere, showing no respect for rank and encouraging servants to inform against their masters. Among the monstrous exactions that Theophanes condemned were Nicephorus' demands that imperial officials pay any back taxes that they had evaded over the past eight years and that those who had evaded inheritance taxes pay them for the past twenty years.[62] When the inspectors were unable to discover how a man had become rich without paying taxes, Nicephorus cleverly ordered him taxed under the law of treasure trove, as if he had found a pot of gold (the order included anyone who actually had).

[59] Theodore, *Patrologia Graeca*, 99: 932A-B.

[60] Theophanes, *Chronographia*, 486. 26–28: "[Νικηφόρος προσέταξε] ἐποπτεύεσθαι πάντας, καὶ ἀναβιβάζεσθαι τὰ τούτων τέλη, παρέχουντας καὶ χαρτιατικῶν ἕνεκα ἀνὰ κερατίων β'." I translate, "[Nicephorus ordered] a survey to be made of all and their taxes to be increased so that they paid two additional keratia [one-twelfth nomisma] each as a roll tax." This interpretation is also that of Bury; see his *History of the Eastern Roman Empire*, 214 n. 1. The passage could conceivably mean that the basic rates were raised as well and that the two keratia were levied on each nomisma of tax, but then the reader would be so likely to misunderstand in Nicephorus' favor that I do not believe Theophanes would have used these words. On all of Nicephorus' measures, see *ibid.*, 212–18; and G. I. Bratianu, *Études byzantines d'histoire économique et sociale* (Paris, 1938), 195–211. Bratianu has suggested that Nicephorus did raise the rates; *ibid.*, 201–02. In general, my interpretations tend to follow those of Bury rather than those of Bratianu.

[61] Although Theophanes reported it under the year 810, the census presumably took more than a year to complete, and I am inclined to believe that it began with the first year of the fifteen-year indiction (assessment) cycle in September 807. Leo III had ordered a partial census in 733, a first year of the indiction; Theophanes, *Chronographia*, 410. Also see note 71, below. Admittedly, however, the significance of the indiction cycle in this period remains obscure. Diocletian's census began in 288, the first year of the indiction, and continued until 302 (under a five-year cycle; the fifteen-year cycle began in 313, under Constantine). See A. H. M. Jones, *The Later Roman Empire, 284–602: A Social, Economic, and Administrative Survey*, 1 (Norman, Okla., 1964): 454.

[62] Theophanes, *Chronographia*, 487. 21–27, 489. 24–28, 487. 9–11. I would translate "τοὺς πένητας" as "[even] the poor," since it is clear that Nicephorus did not treat the rich more favorably. On Nicephorus' failure to respect the rich, see *ibid.*, 486. 19–22, 488. 10.

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The emperor also enforced the collection of port and market duties, which seem to have gone unpaid frequently in the past. Further, he ordered duties to be paid on slaves who were imported without passing the imperial customs station at Abydus and had previously been legally duty free by the luck of geography. Finally, Nicephorus ordered the usual hearth tax to be collected from the tenants of charitable foundations, who had been evading taxation even though they were simply ordinary farmers. In this instance, the hearth tax seems to have been legally due all along, because Nicephorus demanded back payments from the first year of his reign.[63]

Nicephorus also took measures to bring back into production land that had been deserted. He forced a number of people to settle, again without regard to wealth, in the European territories that had been invaded by the Slavs. Although Theophanes' charges of oppression are plausible enough, the economic and political benefits of this resettlement were probably great.[64] The emperor also conscripted bankrupt farmers into the army, assigning their taxes—and presumably their land—to the nearest rich landholder. Furthermore, Nicephorus forced rich shipowners to buy unused land from him at a fixed price; the taxes charged on it then compelled them to bring it under cultivation. Beyond this, the emperor canceled tax exemptions on cultivated land, particularly that belonging to churches and monasteries.[65] Finally, Nicephorus invested some of the empire's gold surplus by forcing the shipowners of Constantinople to take out sizable loans from him at 16⅔ percent interest—an innovation that the emperors of the eighth century seem never to have thought of. Naturally, the shipowners had to expand their trade in order to pay.[66]

Such sweeping reforms must have been motivated by serious abuses. Behind almost every reform previous mismanagement can be detected. The census had been inaccurate and out of date. Officials had abused their power to avoid paying taxes. Rich men had evaded taxation by deceiving or bribing officials or had obtained various legal tax exemptions, probably through influence or bribery. Inheritance taxes and port and market duties in particular had gone unpaid, since legacies and commercial transactions are easier to conceal than land, especially in the absence of good records. Merchants had avoided tariffs by selling slaves before they passed the customs station; secular tenants of charitable institutions had evaded the hearth tax. Much land had been deserted because of enemy raids or the bankruptcy of the landholder. Churches and monasteries had paid little or no taxes on cultivated land. And the gold in the imperial treasury had remained unproductive, the economic consequence of which had been gold deflation.

The shock Theophanes expressed at Nicephorus' reforms shows how accepted

[63] Theophanes, *Chronographia*, 487. 6–9, 488. 8–10, 487. 11–13, 486. 29–487. 2.

[64] *Ibid.*, 486. 8–10. Also see Peter Charanis, "Nicephorus I, The Savior of Greece from the Slavs," *Byzantina-Metabyzantina*, 1 (1946): 75–92.

[65] Theophanes, *Chronographia*, 486. 23–26, 487. 13–16, 486. 28–29, 487. 2–5, 489. 24–27, 487. 17–19.

[66] For this interpretation, see E. Frances, "L'empereur Nicéphore I[er] et le commerce maritime byzantine," *Byzantinoslavica*, 27 (1966): 41–47. Frances has taken issue, I think correctly, with Bury and Bratianu; see Bury, *A History of the Eastern Roman Empire*, 216–17; and Bratianu, *Études byzantines d'histoire économique et sociale*, 208–09.

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these abuses had become, and his indignation suggests that the reforms were in large measure successful. As a monk from a family of rich officials, Theophanes was outraged when Nicephorus abruptly increased the tax burden of the church, the rich, and the officials. When he wrote, of course, the measures were still new, and he may have hoped to persuade the newly crowned Leo V to modify or reverse them. Although Theophanes considered Nicephorus harsh in his financial measures,[67] the emperor does not seem to have raised the basic rates of any taxes. Rather, he applied the taxes equally to all groups, a practice that Theophanes thought was itself outrageous. Raising more revenue more equitably meant fewer bankruptcies and less abandonment of land.

Theophanes praised Michael I Rhangabe (811–13) for his generosity in spending the gold that Nicephorus had amassed, but the *Chronographia* does not indicate that Michael made any changes in the system of taxation.[68] His successors apparently continued Nicephorus' reforms, and, once they became established, no one seems to have found them oppressive. Leo V was praised for his fiscal management, Michael II for temporarily remitting part of the hearth taxes during the civil war of 821–23, and Theophilus for his justice and concern for his subjects' welfare.[69] All three emperors were iconoclasts, yet in this respect they are praised by our iconophile sources. Furthermore, by 856, despite frequent wars and heavy building expenditures, we find the large sum of one hundred and ninety thousand pounds of gold in the treasury.[70]

The difference in the empire's financial administration between the eighth and the ninth century cannot be adequately explained by blaming the policies of the eighth-century emperors. Leo III made at least a halting attempt at a census in 733. Constantine V certainly tried to make the church lands yield revenue by his confiscations. According to Theodore of Studium, at least the port and market duties were strictly collected before Irene's reign.[71] Although Irene's remissions of taxes may have been irresponsible, Theophanes dates them only a year and a half before her overthrow by Nicephorus I, while most of the abuses Nicephorus reformed went back at least eleven more years (the period for which he demanded back taxes), and probably more. To be sure, Nicephorus I was a trained financier and a capable one at that, but his appearance at about this time does not seem to be a pure accident.

BY 802 THE BEGINNING OF NICEPHORUS' REIGN, the revival of learning had been going on for about twenty years. The minuscule hand had some currency, and the

[67] Nicephorus allegedly confiscated certain church lands and then demanded that the church pay taxes on them; he also virtually confiscated the large fortune of a candle seller; Theophanes, *Chronographia*, 487. 30–488. 6. But Theophanes may have suppressed justifying circumstances.

[68] Theophanes, *Chronographia*, 493–94.

[69] On Leo V, see Genesius, *Regum Libri Quattuor*, 1. 16; on Michael II, see Theophanes Continuatus, *Chronographia*, 54; and, on Theophilus, see *ibid.*, 87–88.

[70] For the accumulation of this surplus, see note 7, above.

[71] Theophanes mentioned only Leo's counting male infants, but he may have omitted others who were counted in order to strengthen his comparison of Leo with the Pharaoh of Exodus; *Chronographia*, 410. Also see Theodore, in *Patrologia Graeca*, 99: cols. 929B–933C.

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number of educated Byzantines had risen considerably. In his way Nicephorus I was part of this phenomenon. As a former iconophile civil servant with a good practical education, he worked with the scholarly Patriarch Tarasius and appointed the scholarly Patriarch Nicephorus to office. Nicephorus I naturally benefited from having better-educated civil servants under him, and it is hard to believe that he did not make an effort to appoint well-educated men as his officials, probably while he was still finance minister. It is a reasonable, if unprovable, conjecture that Nicephorus introduced the minuscule hand into the imperial chancery, with the increase in efficiency that that entailed.[72] He waited several years after his accession to make his reforms, which gave time to prepare his civil service for the task. When he had compiled fairly adequate records, he was in a position to demand back taxes, curtail fraud and bribery, and close loopholes in the tax system. (Corruption is, of course, far easier to conceal when records are poor.) Even if the rulers of the eighth century had had Nicephorus' ideas of efficiency—and they apparently did not—they did not have a bureaucracy of the quality that he had and their efforts would have been hampered accordingly. Nicephorus' successors evidently had still better officials. Leo V was praised for appointing "first-rate secretaries" to keep his accounts and provincial officials who did not embezzle.[73]

The effects of rising education and literacy on the army would have been felt more slowly, since changes would have had to spread from the capital to the provinces and reach a group that had less incentive to become educated than civil servants had. Yet, as Browning has observed, many Byzantine military officers were educated—and needed to be. The Byzantine army had to depend largely on written orders and requisitions of supplies, which the illiterate would be unable to prepare or understand, and the semiliterate might misunderstand or prepare incorrectly. As literacy spread, this situation must have improved. The epic hero Digenes Acrites, among other exploits, studied for "three whole years" and learned to read and write. Consequently, when Emperor Basil (I?) wrote to him, the hero understood and was able to write back eloquently.[74] But there must have been times when officers did not understand written orders and were, therefore, of no use in campaigns; such cases must have been more common before the ninth century. Certainly, increased literacy was not the main reason for Byzantine military resurgence in the mid-ninth century; economic recovery and the decline of the caliphate were definitely more important. Still, it is hard to believe that an increase in the functional literacy of the military did not occur at this time and that this increase did not improve military efficiency.

We must be careful not to overdraw the case. Just how much education and literacy spread in Byzantium from the late eighth century cannot be estimated with any precision, nor can just what effect this increase had on the state be

[72] Minuscule was probably too new to have been introduced before Nicephorus' ministry; see Irigoin, "Survie et renouveau de la littérature antique à Constantinople," 288. But it had long been in use for records by the reign of Basil I (867–80); see Browning, "Literacy in the Byzantine World," 51.

[73] Genesius, *Regum Libri Quattuor*, 1. 16.

[74] Browning, "Literacy in the Byzantine World," 42–44; and John Mavrogordato, ed. and trans., *Digenes Acrites*, Grottaferrata version (Oxford, 1956), 70–71, 132–35 (bk. 4, ll. 66–69, 971–1003).

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measured. A comparative study of inscriptions would provide some basis for assessing literacy if the inscriptions were collected systematically and dated, but this has not yet been done. Presumably, eighth-century inscriptions and grafitti had more grammatical errors and misspellings than those of the ninth century; but how great the difference might be is not yet known. Saints' lives can also be exploited further as sources for literacy and education. The collections of letters of the ninth century, particularly those of Theodore of Studium and Ignatius the Deacon, should yield more information when their contents are thoroughly examined. Even the chronicles may contribute something more if studied closely with this question in mind. Naturally, chancery documents of this period would provide the best evidence, and they are not extant. But further study of the Byzantine economy and government, both from later written sources and from coins and seals, should add detail to the picture of all aspects of the middle Byzantine revival.[75]

Much, however, is now known about the origins, nature, and effects of that revival. Beginning with the late eighth century, the educated class in Byzantium, which was also the ruling class, increased its scholarly activity, producing leading scholars of greater distinction, ordinary members of better education, and newly recruited members in greater numbers. In its early stages attributable to the challenge of iconoclasm, this increase in scholarly activity soon gathered independent momentum. As the Byzantine ruling class became better educated, it became better able to perform its administrative functions. This did not necessarily mean that it would do so; it could simply have used its new skills to increase its own wealth and power at the expense of the state. But Emperor Nicephorus I, himself a member of this class and thoroughly familiar with the workings of the government, made a major effort to convert his better-educated civil service into a more efficient one. In this he was largely successful, and his successors continued what he had begun. Thus, in the ninth century the empire's diplomacy was more effective, its taxes were more equitably assessed and more efficiently collected, its army functioned better, and its whole administration was tightened and improved. How much difference these changes made cannot be estimated with any precision, but it seems to have been significant. Combined with other favorable circumstances, the changes led to the revival of the middle Byzantine state.

[75] I am now writing a book on the different aspects of the Byzantine revival between 780 and 842.

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The Characteristics of Plantations in the Nineteenth-Century Sokoto Caliphate (Islamic West Africa)

PAUL E. LOVEJOY

ONE OF THE MOST IMPORTANT UNANSWERED QUESTIONS in economic history today concerns the transformation of systems of slave supply in the nineteenth century. The focus of past research has been on the rapid change in the industrialized centers of Europe and North America and the reorganization of plantation America, where slavery experienced a last burst of energy before the institution collapsed and new patterns of labor emerged. Very little attention has been directed at the corresponding changes in Africa during this period, despite many relatively unexplored assumptions about the nature of African society and economy on the eve of European colonialism. This article focuses on this neglected theme by concentrating on the developments in one West African state, the Sokoto Caliphate, and by exploring the importance of its plantation economy. The pattern of slave use found in the Sokoto Caliphate was characteristic of many parts of Africa in the nineteenth century, and the growth of this sector was related to the decline of the transatlantic slave trade and the corresponding availability of slaves for use within Africa.

The relationship between internal African developments and the foreign slave trade, including the European portion across the Atlantic, was crucial in maintaining slavery as a viable institution within Africa. The supply of slaves to European and other markets promoted institutions of enslavement and marketing that maintained a steady supply of cheap slaves. As Europe withdrew from the transatlantic trade, the availability of slaves for domestic use enabled economic expansion on the regional level within Africa. Along the coast, this expansion was tied to the export of agricultural and other products to Europe.[1] In the interior, the regional pull was related to ecologically based trade along the southern

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[1] A. G. Hopkins, *An Economic History of West Africa* (London, 1973); and Martin Klein, "Slavery, the Slave Trade, and Legitimate Commerce in Late Nineteenth-Century Africa," *Études d'histoire africaine*, 2 (1971): 5–28.

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edge of the Sahara Desert.[2] Plantations were a major part of this economic expansion.

The supply of cheap slaves for markets in the Americas and in North Africa continued well into the nineteenth century.[3] David Eltis has shown that slave exports from Africa remained at a high level until at least the early 1840s.[4] Indeed, his projection is that the figure was probably some 30 percent higher than Philip D. Curtin allowed in his pioneering census of the trade for the period 1820–43. Similarly, David Northrup and Patrick Manning have examined the export figures for the Bights of Biafra and Benin during this period and have confirmed that slave exports continued to be a significant dimension of the West African economy until the 1840s.[5] Ralph Austen's survey of trans-Saharan slave exports in the nineteenth century indicates a similar conclusion, and this trade did not enter a period of decline until much later in the century.[6] The results of these quantitative studies help demonstrate why slavery remained a dynamic force within West African society and economy during the nineteenth century. External demand provided one incentive for the enslavement of people within West Africa, and—even when the external transatlantic trade declined after the 1840s, indeed falling very quickly thereafter—the internal supply mechanism continued to function. For such areas as the northern savanna, moreover, the trans-Saharan market helped minimize the ending of the European trade, which had been of secondary importance anyway. The external impact of the slave trade had its effect, therefore, but probably as important, if not more so, was the growth of internal African demand for slaves that was able to keep abreast of slave supply. In effect, the institutionalization of the enslavement process and the slave trade encouraged a reliance on slave labor domestically.[7]

[2] Paul E. Lovejoy, "Indigenous African Slavery," paper presented at the Slave Studies Conference: Directions in Current Scholarship, University of Waterloo, Waterloo, Ontario, March 1979, and forthcoming in *Historical Reflections/Reflexions historiques*, 6 (1979).

[3] The scholarly discussion of the quantity of slave exports from West Africa is based on Philip D. Curtin, *The Atlantic Slave Trade: A Census* (Madison, Wisc., 1969), and numerous revisions by Curtin and others of these estimates. For the nineteenth century, see especially E. Philip LeVeen, *British Slave Trade Suppression Policies, 1821–1865* (New York, 1977), and "A Quantitative Analysis of the Impact of British Suppression Policies on the Volume of the Nineteenth-Century Atlantic Slave Trade," in Stanley L. Engerman and Eugene D. Genovese, eds., *Race and Slavery in the Western Hemisphere: Quantitative Studies* (Princeton, 1975), 51–81; Philip D. Curtin, "Measuring the Atlantic Slave Trade," in *ibid.*, 107–28; David Northrup, "The Compatibility of the Slave and Palm Oil Trades in the Bight of Biafra," *Journal of African History* [hereafter *JAfH*], 17 (1976): 353–64; Herbert S. Klein, *The Middle Passage: Comparative Studies in the Atlantic Slave Trade* (Princeton, 1978); David Eltis, "The Direction and Fluctuation of the Transatlantic Slave Trade, 1821–43: A Revision of the 1845 Parliamentary Paper," in H. A. Gemery and J. S. Hogendorn, eds., *The Uncommon Market: Essays in the Economic History of the Atlantic Slave Trade* (New York, 1979), 273–301, "The Export of Slaves from Africa, 1821–1843," *Journal of Economic History*, 37 (1977): 410–15, and "The Transatlantic Slave Trade, 1821–1843" (Ph.D. dissertation, University of Rochester, 1978); Patrick Manning, "The Slave Trade in the Bight of Benin, 1640–1890," in Gemery and Hogendorn, *The Uncommon Market*, 107–41, and "Growth despite Slavery and Taxes: The Bight of Benin, 1640–1960," paper presented at the annual meeting of the African Studies Association, Baltimore, 1978.

[4] Eltis, "The Transatlantic Slave Trade, 1821–1843," 24–113, esp. 52–56.

[5] Northrup, "Slave and Palm Oil Trades in the Bight of Biafra," 354–58; and Manning, "The Slave Trade in the Bight of Benin, 1640–1890," 116–19, and "Growth despite Slavery and Taxes: The Bight of Benin, 1640–1960."

[6] Austen, "The Trans-Saharan Slave Trade: A Tentative Census," in Gemery and Hogendorn, *The Uncommon Market*, 23–76.

[7] For general surveys, see Lovejoy, "Indigenous African Slavery"; Martin Klein and Paul E. Lovejoy, "Slavery in West Africa," in Gemery and Hogendorn, *The Uncommon Market*, 181–212; and Frederick Cooper, "The Problem of Slavery in African Studies," *JAfH*, 20 (1979): 103–25.

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One characteristic of continued exports and the intensified use of slaves domestically related to relatively stable prices for slaves; the price of slaves in some areas, moreover, appears to have declined in the course of the century. The significance of this market situation was that even areas that were relatively far from the coastal entrepots were still affected. This was the case for the Sokoto Caliphate, for example, where the portion of slaves exported to the Americas was never very large. Most slaves there were either exported across the Sahara, sold to other African states, or used domestically. Nonetheless, the price structure of the slave market in the Sokoto Caliphate, as the series constructed by David Tambo has shown, demonstrates the wider West African pattern. The price of slaves ranged between 15 and 67 Maria Theresa dollars (MT $) for young female slaves and from MT $5–40 for young males. The average mean price for young females was about MT $26 before mid-century and about MT $35 afterwards, while for young males the comparable figures were MT $14 and MT $19. When monetary inflation is taken into account, the real price for slaves actually declined, since monetary values fell by over 50 percent during the course of the century.[8]

Eltis has shown that the price of slaves on the West African coast declined from an average of $66 between 1821 and 1837 to an average of $61 between 1838 and 1843.[9] Thereafter, the coastal price must have dropped considerably, although there has been no study of the problem. Even the price drop in the period before the 1840s probably had only a marginal effect on the far interior, for a comparison with Tambo's figures for the Sokoto Caliphate shows a considerable mark-up, on the order of over 100 percent above either of Eltis's figures. The price rise in the Sokoto Caliphate after mid-century, although possibly representing a decline in real price, is likely to have brought the Sokoto Caliphate more into line with coastal prices. Certainly, the line of enquiry that Tambo and Eltis initiated needs to be explored much more fully.

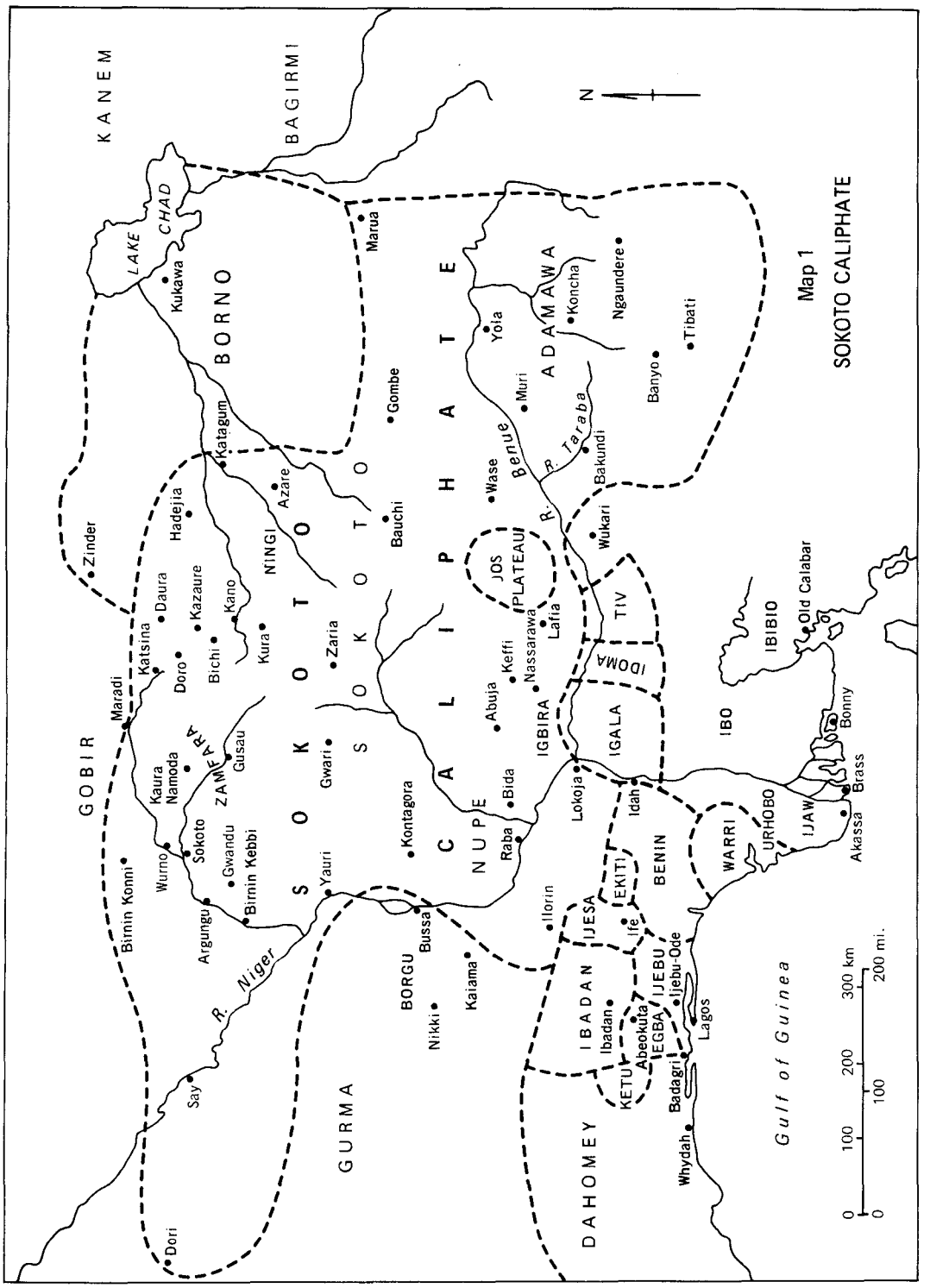
The link between such distant places as the Sokoto Caliphate and the transatlantic slave trade, demonstrated in part by a similar market situation in which slaves were plentiful, was related to the peculiar dimension of slave marketing and to the general organization of long-distance luxury commerce in precolonial Africa. The crucial feature of long-distance trade was the transfer of goods over great distances in order to take advantage of ecological differences within Africa.[10] Thus, trade was essentially a luxury trade. Slave marketing was different in that transport was not as much of a problem, since slaves could move themselves. The advantage in slave marketing, moreover, related to the transfer of individuals from areas close to their homes to distant markets where escape would be more difficult. In this sense, the slave trade corresponded to other aspects of long-distance trade. Value increased with distance; but, unlike goods based on ecological advantage that tended to flow from areas of supply to mar-

[8] Tambo, "The Sokoto Caliphate Slave Trade in the Nineteenth Century," *International Journal of African Historical Studies*, 9 (1976): 187–217. For a discussion of monetary inflation, see Paul E. Lovejoy, "Interregional Monetary Flows in the Precolonial Trade of Nigeria," *JAfH*, 15 (1974): 563–85.

[9] Eltis, "The Transatlantic Slave Trade, 1821–1843," 324.

[10] Paul E. Lovejoy and J. S. Hogendorn, "Slave Marketing in West Africa," in Gemery and Hogendorn, *The Uncommon Market*, 213–35.

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kets where such commodities were not produced, slaves tended to flow in all directions. Consequently, even though such interior states as the Sokoto Caliphate were not large contributors to the transatlantic slave trade, major changes in that trade, particularly its demise, affected the Sokoto Caliphate as well. The general impact of European abolition, therefore, spread inland. Decreased demand for slaves at the coast freed slaves for use within local states. The availability of relatively cheap slaves made possible regional economic expansion based on slave labor.

THIS INTENSIFIED USE OF SLAVES within Africa resulted in a wide variety of practices. Slaves were used as porters, canoemen, craftsmen, farmers, miners, soldiers, government officials, domestic servants, concubines—virtually in all aspects of the economy and society. But my concern is with the development of plantation slavery. The existence or absence of plantations is a controversial topic that is, in part, a problem of definition. Here "plantation" is used to refer to large-scale agriculture employing slave labor, often organized in gangs. Such organization depended upon the existence of individuals who had access to land and labor or who commanded the capital that could acquire these factors. The term is used to distinguish the cultivation of land in large aggregates from small-scale farming by peasants. Such practices, which involved outputs that more than fed the slave producers, required a distributional system for agricultural surplus, either through the market mechanism or some comparable means of redistribution. These characteristics of agriculture, similar to those found in plantation economies elsewhere, make the adoption of "plantation" as a descriptive term appropriate here.[11]

There were many differences between plantation agriculture in Africa and plantations in the Americas, where the term is used most often and without reservation. First, market forces were weaker in Africa, and they were often regional rather than intercontinental. Second, the ideological framework for plantation slavery was very different from European norms. Race was not significant, although cultural and other differences were often emphasized. Slave codes were tied to kinship patterns and, where Islam was important, to Muslim tradition and law. Third, the wide range of uses for slaves meant that a distinct class of slaves was less fully developed than in the Americas, even though plantation slaves were often more fully isolated from society than other slaves. Finally, emancipation of slaves was more common in Africa, and this dimension was im-

[11] The use of "plantation" should not be construed as an assertion that institutions in Africa and the New World were identical. Rather, I mean to draw attention to such issues in the study of Africa as the extent of market development and the degree of social stratification. The comparison with the Americas is implicit in virtually all studies of slavery in Africa, but usually the wide variety of servile relationships is used to demonstrate the striking differences from American patterns. For a survey of slavery in West Africa, see Klein and Lovejoy, "Slavery in West Africa," 181–212; Cooper, "The Problem of Slavery in African Studies," 103–25; Igor Kopytoff and Suzanne Miers, "African 'Slavery' as an Institution of Marginality," in S. Miers and I. Kopytoff, eds., *Slavery in Africa: Historical and Anthropological Perspectives* (Madison, Wisc., 1977), 3–81; and Allan Fisher and Humphrey Fisher, *Slavery and Muslim Society in Africa* (London, 1970).

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portant in slave control and the ability of individual slaves to accommodate themselves to their position in society.

These similarities and differences that characterize plantation slavery in Africa suggest that these plantations should be viewed comparatively, although care must be taken not to force the African experience into an unwarranted connection with the legacy of slavery in the Americas. In short, "plantation" is not used here pejoratively but in order to promote a closer analysis of change within Africa and to suggest that this change was *distantly* related to the Americas. My approach is meant to be seen within the comparative framework advanced by numerous scholars who have examined plantations in the Americas,[12] but it adds to that framework by drawing attention to the existence of plantations elsewhere. More specifically, it considers only plantation development within Islamic Africa; other scholars can consider the similarities and differences of plantations in non-Muslim societies.

In identifying an Islamic pattern of plantation agriculture in Africa, this analysis follows the lead of Frederick Cooper, who has analyzed plantation development along the East African coast in the nineteenth century.[13] There, immigrant Arabs from Oman and local Swahili invested heavily in slaves in order to grow cloves, coconuts, grain, and other goods for export. The most important plantation centers were on the islands of Zanzibar and Pemba and along the mainland coast, especially around Malindi and Mombasa. Although slaves worked in a regimented fashion characteristic of other plantation systems, various Islamic practices tempered the institution and thereby helped provide a distinctive feature to this plantation system. These practices included extensive concubinage, the use of slaves in the military, emancipation of slaves according to Muslim custom, and the propagation of Islam among slaves. In short, despite the pull of market forces in the Indian Ocean, the institution of slavery and, therefore, the plantation system developed their own characteristics.

The similarity between plantation slavery on the East African coast and in the Islamic northern savanna is only beginning to be explored. This article at-

[12] The debate over the various theoretical approaches to the study of plantations in the Americas is now extensive. I have been influenced by the following interpretations: Eugene D. Genovese, *The Political Economy of Slavery* (New York, 1965); Frederick Cooper, *Plantation Slavery on the East Coast of Africa* (New Haven, 1977), 2–20; Jay R. Mandle, "The Plantation Economy: An Essay in Definition," *Science and Society*, 36 (1972): 49–62; Julian Steward, "Perspectives on Plantations," in Vera Rubin *et al., Plantation Systems of the New World* (Washington, 1959), 5–12; Ida C. Greaves, "Plantations in World Economy," in *ibid.*, 13–23; Sidney Mintz, "The Plantation as a Socio-Cultural Type," in *ibid.*, 42–59; Eric R. Wolf and Sidney Mintz, "Haciendas and Plantations in Middle America and the Antilles," *Social and Economic Studies*, 6 (1957): 380–412; John V. Lombardi, "Comparative Slave Systems in the Americas: A Critical Review," in Richard Graham and Peter H. Smith, eds., *New Approaches to Latin American History* (Austin, 1974), 156–74; Sidney Greenfield, "Slavery and the Plantation in the New World," *Journal of Inter-American Studies*, 11 (1969): 44–57; Charles Wagley, "Plantation America: A Culture Sphere," in Vera Rubin, ed., *Caribbean Studies: A Symposium* (Seattle, 1957), 3–13; Franklin W. Knight, *Slave Society in Cuba during the Nineteenth Century* (Madison, Wisc., 1970); and Marion Malowist, "Les débuts du système de plantations dans la période des grandes découvertes," *Africana Bulletin*, 10 (1969): 9–18. Also see Vera Rubin and Arthur Tuden, eds., *Comparative Perspectives on Slavery in New World Plantation Societies*, New York Academy of Sciences, no. 292 (New York, 1977), esp. Philip D. Curtin, "Slavery and the Empire," 3–11.

[13] Cooper, *Plantation Slavery on the East Coast of Africa*. Also see Cooper, "The Problem of Slavery in African Studies," 112–25, and his forthcoming book on the abolition of slavery in East Africa and the transition to other forms of labor.

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tempts to examine the details of one state in the northern savanna in order to reach a greater understanding of the relationship between plantation agriculture and slavery in Islamic society; but it is important to the argument that the extent of similar practices in the northern savanna be recognized. A brief survey of this vast region suggests this similarity but also reveals the scope of the analytical problem. Such a survey demonstrates that in many places slavery was exceedingly common, with slaves often constituting a majority of the population. Moreover, slaves were used extensively in agriculture, frequently in large concentrations, although the equation of these concentrations with "plantations," an analytical convenience adopted here, is not always accepted and is sometimes specifically denied. Thus, the problem discussed in this article is of particular significance in the reconstruction of African history. If the following analysis proves useful, then more detailed study of the various parts of Islamic Africa is required to determine the extent to which a plantation sector was important. The analysis of the Sokoto Caliphate is meant as a detailed study of the problems such a broad re-interpretation of African history involves.

That slaves constituted a significant percentage of the population of Muslim areas is now generally accepted. Numerous studies of individual areas have consistently revealed high concentrations of slaves, ranging from as much as half to over three-quarters of the total population. Much of this analysis rests on the detailed colonial enquiries of the French and British administrations in the first decade of the twentieth century,[14] but other information based on interviews in specific locations has confirmed these data. It is not surprising, therefore, that slaves were used extensively in agriculture. As Mamadou Saliou Baldé has stated for Futa Jallon, "En effet, les esclaves, dans leur immense majorité, n'etaient utilisés qu'a cultiver les champs." Similarly, E. Pollet and G. Winters, in their description of the Soninke center of Dyahunu, in the sahel of western Mali, stated that "slave labour, . . . constitute[d] the main element of the agricultural labour force and as such represented the economic base of the society."[15] Other remarks of this sort apply to many places.[16]

The nature of agricultural slavery is more open to dispute, however. William Derman has referred to the servile workers of Futa Jallon as "serfs," and Marion Johnson, in her description of similar agricultural organization in Masina, has employed the same comparison with serfdom.[17] The problem with this identifi-

[14] For a brief survey of these materials, see Lovejoy, "Indigenous African Slavery"; Martin Klein, "Slave Systems in the Western Sudan" (forthcoming); J.-L. Boutillier, "Les captifs en A.O.F. (1903–1905)," *Bulletin de l'I.F.A.N.*, sér. B, 30 (1968): 513–35; and John Grace, *Domestic Slavery in West Africa* (New York, 1975).

[15] Baldé, "L'esclavage et la guerre sainte au Fouta Jalon," in Claude Meillassoux, ed., *L'esclavage en Afrique précoloniale* (Paris, 1975), 201; and Pollet and Winters, "The Social Organisation of Agricultural Labour among the Soninke (Dyahunu, Mali)," in David Seddon, ed., *Relations of Production: Marxist Approaches to Economic Anthropology* (London, 1978), 333.

[16] See, for example, Claude Meillassoux, "État et conditions des esclaves à Gumbu (Mali) au XIX[e] siècle," *JAfH*, 14 (1973): 431–40; Richard Roberts, "The Maraka and the Economy of the Middle Niger Valley, 1790–1908" (Ph.D. dissertation, University of Toronto, 1978), 280–93, and "Long-Distance Trade and Production: Sinsani in the Nineteenth Century," paper presented at the annual meeting of the African Studies Association, Baltimore, 1978; and L. O. Sanneh, "Slavery, Islam, and the Jakhanke People of West Africa," *Africa*, 46 (1976): 80–97.

[17] Derman, *Serfs, Peasants, and Socialists* (Berkeley and Los Angeles, 1973); and Johnson, "The Economic Foundations of an Islamic Theocracy: The Case of Masina," *JAfH*, 17 (1976): 488–89.

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cation is that the servile population was often recently settled—Futa Jallon in the mid-eighteenth century and Masina from the second quarter of the nineteenth century. Additions to the servile population were common, and, despite social pressures to the contrary, individuals could be sold. Furthermore, the relationship between land and agricultural workers was fundamentally different from that under serfdom. Land was readily available, and people were not attached to the land. Finally, the servile institution was perceived within an Islamic framework in which the political and economic elite accepted Islamic law (*shari'a*) as the final arbitrator in cases of servility, and the *shari'a* recognized slavery.

Another problem relates to the nature of agricultural settlement. Baldé has called the same slave communities in Futa Jallon that Derman identified as serf villages, "hameau d'esclaves."[18] J. L. Boutillier, in his description of the commercial center of Buna (Upper Volta), for example, has noted, "Pour le plus grand nombre [d'esclaves], les captifs travaillant dans ce secteur ne residaient pas dans la ville même de Bouna aux côtes de leurs maîtres mais dans des campements de culture *djoso*, c'est-à-dire, littéralement, village de captifs. . . ."[19] Thus, slaves lived in "hamlets" and "villages," but the identification of these as "plantations" has been avoided or denied. This pattern in the scholarship of the northern savanna is found not only in West Africa but also much further east. Rex S. O'Fahey, for example, in his examination of slavery in Dār-Fūr (Republic of Sudan), noted that "slaves were also employed by the sultans and others as settled agricultural labourers, although the evidence for a form of 'plantation' slavery is meagre," despite the fact that the use of slaves as agricultural laborers "was widespread," especially on estates.[20]

The term "plantation" has not been altogether avoided. In his study of the Maraka commercial centers of Mali, Richard Roberts has found the concept useful in depicting the extensive number of agricultural settlements owned by local aristocrats and merchants, and Dennis Cordell, in his study of Dar Al-Kuti, a small Muslim state located in northern Central African Republic, has also accepted the usefulness of the concept.[21] The issue, therefore, is clear. Is the institution being described for these areas of the northern savanna one involving plantations or something else? For the Sokoto Caliphate, at least, "plantations" were an important dimension of the economy and society.

THE SOKOTO CALIPHATE was geographically the largest and demographically the most populous state in nineteenth-century Africa south of the Sahara. It incor-

[18] Baldé, "L'esclavage et la guerre sainte au Fouta Jalon," 202.

[19] J.-L. Boutillier, "Les trois esclaves de Bouna," in Meillassoux, *L'esclavage en Afrique précoloniale*, 268.

[20] R. S. O'Fahey, "Slavery and the Slave Trade in Dar Fur," *JAfH*, 14 (1973): 37–38. Also see R. S. O'Fahey, "Slavery and Society in Dar Fur," paper presented at the conference on Islamic Africa: Slavery and Related Institutions, Princeton University, 1977.

[21] Roberts, "The Maraka and the Economy of the Middle Niger Valley, 1790–1908," 307–20; Dennis Cordell, "Dar Al-Kuti: A History of the Slave Trade and State Formation on the Islamic Frontier in Northern Equatorial Africa (Central African Republic and Chad) in the Nineteenth and Twentieth Centuries" (Ph.D. dissertation, University of Wisconsin, 1977), 236–68; and a personal communication with Dennis Cordell.

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porated much of what is now northern Nigeria and parts of Upper Volta, Niger, and Cameroun, although there were scattered areas within its boundaries that were autonomous. The state was founded as a result of an Islamic holy war (*jihad*), principally between 1804 and 1808 but continuing in some areas for several decades.[22] Administratively, the state was relatively decentralized. In 1817 problems over succession resulted in the recognition of twin capitals, at Gwandu and Sokoto, and the division of the caliphate into eastern and western divisions. Nonetheless, the proximity of these two towns and close relations within the ruling aristocracy still provided central direction in governing the empire. The caliphate was subdivided into emirates, centered on walled towns. The more important of these—including Kano, Katsina, Zaria, Fombina, Muri, Nupe, Ilorin, and Bauchi—had numerous subemirates and dependent towns under their supervision; and there were many other autonomous but small emirates that owed direct homage to Sokoto or Gwandu. The population of this vast state cannot be determined with any accuracy, although Polly Hill's estimates provide some indication of size in circa 1900.[23] Based on early tax assessments and much guesswork, it appears that something like twenty million people lived in the Sokoto Caliphate, with perhaps half of the total in the central emirates (Kano, Katsina, and Zaria) and the capital districts (Gwandu and Sokoto).

The Islamic heritage provided the political and legal framework for the caliphate's society and economy. The foundation of the state through *jihad* associated the new aristocracy with a long tradition of Islamic reform in West Africa, and this tradition remained an inspiration throughout the nineteenth century. The writings of the early leaders—Shehu Usuman dan Fodio, his brother Abdullahi, and his son Muhammad Bello, all of whom wrote many books and pamphlets on history, religion, government, and law—demonstrate the vitality of the Islamic heritage. As this literature indicates, caliphate society identified with the propagation of Islam, the promotion of religious learning, the extension of Islamic law, and the consolidation of a theocracy.[24] Such a conception of society and government reinforced a well-stratified social structure. Those closely associated with Usuman dan Fodio and the original leaders of the *jihad* successfully established themselves as an aristocracy; admission to highest office was possible only for the direct descendants of these first rulers, although slaves were

[22] For a history of the Sokoto Caliphate, see Sa'ad Abubakar, "The Emirate-Type of Government in the Sokoto Caliphate," *Journal of the Historical Society of Nigeria*, 7 (1974): 211–29; M. Z. Njeuma, "The Foundations of Pre-European Administration in Adamawa: Historical Considerations," *ibid.*, 7 (1973): 3–15; S. A. Balogun, "Succession Tradition in Gwandu History, 1817–1918," *ibid.*, 17–48; Paul Irwin, "Visions of Times Past: The Oral Histories of Liptako," author's typescript (I wish to thank Irwin for permission to cite this study); R. A. Adeleye and C. C. Stewart, "The Sokoto Caliphate in the Nineteenth Century," in J. F. A. Ajayi and Michael Crowder, eds., *History of West Africa* (2d ed., London, forthcoming); Murray Last, *The Sokoto Caliphate* (London, 1967); R. A. Adeleye, *Power and Diplomacy in Northern Nigeria, 1804–1906* (London, 1971); Adamu Fika, *The Kano Civil War and British Overrule* (Oxford, 1978); and Peter Waterman, "The Jihad in Hausaland as an Episode in African History," *Kroniek van Afrika*, 2 (1975): 141–52.

[23] Hill, *Population, Prosperity, and Poverty: Rural Kano, 1900 and 1970* (Cambridge, 1977), 17–19. I have made allowance here for the fact that Hill based her estimate on colonial northern Nigeria, which includes Borno and other areas outside the Sokoto Caliphate but excludes areas of the caliphate that were not included in British Nigeria.

[24] For an excellent survey of this literature and the Islamic heritage, see Last, *The Sokoto Caliphate*, xxv–xliv, lxxvi–lxxx, 3–22, 207–22.

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also appointed to specific positions, in keeping with Islamic traditions of government. The learned class (*'ulema*) was more open, since admission to its ranks depended upon piety and knowledge of classical Islamic texts and the literature of the *jihad*. Merchants, craftsmen, livestock herders, and other occupational designations were also recognized as separate social categories, while many subject people with no or only marginal Islamic credentials were identified by ethnic names.[25] Unless these subject populations paid tribute, they were liable to slave raids and other punitive measures. Slaves, of course, were a major social category, and their origins to a great extent were from among the dependent peasant population or from areas beyond the frontiers of the empire. So many people were servile that the enslaved and their descendants had a wide spectrum of occupational and social possibilities, including military recruitment, political appointment, craft training, concubinage, and agricultural employment. Those who worked the plantations, therefore, represented only a portion of the slaves, although the size and importance of the plantation sector meant that they constituted a large percentage of the servile population.

The Sokoto Caliphate was located in the savanna and was part of a regional economy that straddled the southern edge of the Sahara Desert.[26] Specialization within this region was extensive, with commodities transported along north-south corridors that took advantage of ecological differences. Cotton, indigo, tobacco, grain, other foodstuffs, livestock, salt, ivory, slaves, kola nuts, and manufactures (including domestic textiles, leather goods, ironware) as well as imported commodities headed the list of products traded over great distances. Textile manufacturing, based on cottage production and involving thousands of dye pits, was centered in Kano and Zaria Emirates, although production on a much smaller scale was widely scattered, most notably in Nupe and Ilorin. Dozens of varieties of salt—used in recipes, medicinal formulas for humans and livestock, leather tanning, textile production, soap making, and additives to chewing tobacco—were imported from the neighboring state of Borno. Ivory came from the southeastern emirates, while leather goods (including shoes, cushions, and bags) were made in Sokoto, Katsina, Kano, and elsewhere. A common currency, based on cowries and Maria Theresa dollars, served this commerce, and credit facilities operated throughout the breadth of the caliphate and beyond. Although not as significant as the volume and value of internal trade, the links with the outside world were, nonetheless, crucial. These links included trade across the Sahara in ivory, hides and skins, ostrich feathers, and slaves. Also important was exchange with other parts of West Africa, particularly Asante, the source of imported kola nuts. There was some direct trade with Europe after mid-century, when the Niger River was opened to steamships. Every dimension of this regional economy was important to the plantation sector, which produced cotton, indigo, grain, and other goods for export and catered to the nu-

[25] M. G. Smith, "The Hausa System of Social Status," *Africa*, 29 (1959): 239–52.

[26] For some dimensions of the Central Sudanese economy, see Paul E. Lovejoy, "Plantations in the Economy of the Sokoto Caliphate," *JAfH*, 19 (1978): 341–68, and "Interregional Monetary Flows in the Precolonial Trade of Nigeria," 563–85; and Paul E. Lovejoy and Stephen Baier, "The Desert-Side Economy of the Central Sudan," *International Journal of African Historical Studies*, 8 (1975): 551–81.

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merous caravans that transported commodities from one end of the caliphate to the other.

SEEN IN WORLD PERSPECTIVE, virtually all agricultural practices in West Africa, including the Sokoto Caliphate, were labor intensive. The concentration of slaves did not result in any significant improvement in this situation; there were, at least, no obvious economies of scale that favored the use of large numbers of slaves instead of the reliance on peasant production. The advantage to slave owners related to their ability to extract surplus from agricultural output; and, as a consequence of this aim, many characteristics of Sokoto Caliphate agriculture resembled plantation economies elsewhere. These included (1) the predominance of slave labor, often organized in gangs; (2) the presence of merchants and aristocrats who had access to land and labor or who commanded the capital to acquire them; (3) the availability of cultivatable land that could be farmed in large aggregates, in contrast to small, peasant holdings; and (4) the operation of a distributional system for agricultural output, both through the market mechanism and other means of redistribution. The main distinguishing characteristics of these plantations related to the importance of the regional market, the ideological and legal setting based on Islam, and the complexity of a well-stratified society with a tremendous diversity within the institution of slavery.

"Plantation" has been used to describe agriculture in the Sokoto Caliphate by a number of authors, but there is far from general acceptance of the term's applicability. M. G. Smith considered the large slave estates of Zaria Emirate to be plantations, and J. S. Hogendorn and I have continued this orientation. In our work Steven Baier and I have considered the slave estates along the desert edge and the agricultural investments of merchants around the Hausa cities in terms compatible with a plantation analysis. This analysis emphasizes the regional dynamic in plantation development, which suggests an important difference from the role of American plantations in the European world-economy. Michael Mason has used plantation as a descriptive term for Nupe Emirate, although his use of "captive labour" to denote slaves suggests that he has recognized significant differences from the Americas.[27] Indeed, all of these scholars have been as impressed by the differences with American patterns as with the similarities. The comparison is implicit, but weaker market forces in the Central Sudan and

[27] Smith, "Introduction," in Mary Smith, *Baba of Karo: A Woman of the Moslem Hausa* (New York, 1955), 11–34, "Slavery and Emancipation in Two Societies," in Smith, *The Plural Society in the British West Indies* (Berkeley and Los Angeles, 1965), 116–61, and *The Economy of Hausa Communities of Zaria* (London, 1955), 81–82, 102–08; Hogendorn, "The Economics of Slave Use on Two 'Plantations' in the Zaria Emirate of the Sokoto Caliphate," *International Journal of African Historical Studies*, 10 (1977): 369–83; Baier and Lovejoy, "The Tuareg of the Central Sudan: Gradations in Servility at the Desert Edge (Niger and Nigeria)," in Miers and Kopytoff, *Slavery in Africa*, 391–411; Lovejoy, *Caravans of Kola: Hausa Trade with Asante, 1700–1900* (Zaria, forthcoming); and Mason, "Captive and Client Labour and the Economy of the Bida Emirate, 1857–1901," *JAfH*, 14 (1973): 453–71, and "Trade and State in Nineteenth-Century Nupe," paper presented at the Seminar on the Economic History of the Central Savanna of West Africa, Kano, 1976 [hereafter Kano Seminar, 1976]. Also see my "Plantations in the Economy of the Sokoto Caliphate," 341–68.

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the mediating role of Islam constitute special dimensions of the political economy that distinguish caliphate agriculture.

The plantation as a by-product of a strong militaristic tradition within West Africa constitutes a further special dimension. For the savanna region, this tradition was part of Islamic expansion and consolidation based on slave raiding, war, trade, and the massive employment of slaves in agriculture, mining, households, and even the military. In his analysis of warfare in the Sokoto Caliphate, for example, Joseph P. Smaldone has argued,

> Slave farms in particular were important forms of capital investment and insurance for the wealthy. During the nineteenth century there was a remarkable increase in the number of such slave plantations. This increase was attributable not only to the availability of war captives, but also to the Hausa laws of property inheritance. Since these laws provided for the division of the estate of the deceased man among his sons, the size of any inheritance was reduced accordingly. Such practices encouraged competition over inheritance rights and further slave raids and purchases.[28]

In fact, large-scale agricultural output had been a feature of the economy of the northern savanna since the epoch of the great medieval empires, a situation that was related to the peripheral relationship of sub-Saharan Africa to the Islamic world and its demand for slaves.[29] Massive enslavement inevitably meant that captives would be put to work within the enslaving states, at least temporarily until they could be exported. Thus, nineteenth-century patterns in the Sokoto Caliphate can be considered a direct outgrowth of established institutions within West Africa. Plantations had been a part of the economy for centuries, and continued enslavement logically meant that many slaves would be concentrated in the countryside as agricultural workers.

Despite this tradition of identifying large-scale agriculture with plantations, that a comparison with the Americas is warranted or that the Islamic tradition included plantations has not always been recognized. Yusufu Bala Usman and others have used Hausa terminology and have not addressed themselves to the questions of market development and the implications of large-scale agricultural enterprise. Murray Last, Saad Abubakar, and others have minimized the importance of plantations, either by failing to discuss them or by treating them in a cursory manner.[30] In all of these cases there appears to be a reluctance to ex-

[28] Smaldone, *Warfare in the Sokoto Caliphate* (London, 1977), 148.

[29] For a model for treating slavery, including plantation slavery, as an outgrowth of this militaristic tradition, see Terray, "Long Distance Exchange and the Formation of the State: The Case of the Abron Kingdom of Gyaman," *Economy and Society*, 3 (1974): 315–45, and "Classes and Class Consciousness in the Abron Kingdom of Gyaman," in Maurice Bloch, ed., *Marxist Analyses and Social Anthropology* (London, 1975), 85–136; and Meillassoux, "The Role of Slavery in the Economic and Social History of Sahelo-Sudanic Africa," paper presented at the Kano Seminar, 1976. Mason has adopted a similar analysis for Nupe; see his "Trade and State in Nineteenth-Century Nupe." For a summary of the Terray-Meillassoux model, see Klein and Lovejoy, "Slavery in West Africa," 183, 207–10.

[30] Usman, "The Transformation of Katsina, c. 1796–1903: The Overthrow of the *Sarauta* System and the Establishment and Evolution of the Emirate" (Ph.D. dissertation, Ahmadu Bello University, 1974); Garba Nadama, "Legends, Rituals, and Ceremonies Connected with Agricultural Activity and Craft Production in Zamfara," paper presented at the Kano Seminar, 1976; Last, *The Sokoto Caliphate*, 78–80, 102–04, and "An Aspect of Muhammad Bello's Social Policy," *Kano Studies*, 1 (1966): 56–59; and Abubakar, "A Survey of the Economy of the Eastern Emirates of the Sokoto Caliphate in the Nineteenth Century," paper presented at the Third Annual Seminar, Departments of History, Ahmadu Bello University and Abdullahi Bayero College, Sokoto, 1975.

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amine practices that might reflect adversely on the Islamic legitimacy of the caliphate leadership. This approach avoids the problem of analyzing the plantation as a major Islamic institution.

Polly Hill has been perhaps the most serious critic of those scholars (particularly of M. G. Smith) who have looked at the plantation sector, although her position with respect to the Islamic plantation tradition and the comparison with the Americas is not altogether clear.[31] Her main argument is that small-scale farming, often employing a few slaves, was the dominant form of agriculture in the central parts of the caliphate in the nineteenth century. Consequently, she has preferred "farm-slavery" as a descriptive term, thereby suggesting that plantations were rare, if they existed in Kano and Katsina at all. She has also made a distinction between Hausa and Fulani institutions, as if there were two separate societies in the Sokoto Caliphate when, in fact, such a distinction relates more to economic specialization and class stratification. By denying that "Fulani" estates and slave villages were common in Kano and Katsina, she has concluded that "Hausa" agricultural practice was small in scale, even when her own data suggest otherwise: her detailed examination of one small rural area adjacent to Kano city reveals that thirteen slave owners possessed hundreds of slaves among them and that most of these owners were nonresident planters who lived in Kano city. Moreover, many agricultural estates were owned by the aristocracy in Kano and Katsina, although it is likely that merchants owned the greatest number of plantations. To distinguish between Hausa and Fulani institutions is perhaps useful for some purposes but it clouds the main issue.

Much of the confusion stems from the lack of a single term in the languages of the caliphate that can be translated into English as "plantation." The closest terms are *rinji* or *runji* (pl. *rumada*) and *gandu*. The first, from the Fulfulbe *rumde*, was common in Zaria, Daura, and elsewhere, but may have been unknown to many Hausa speakers. *Rinji* has been defined as (1) "Fulani slaves and dependents who live together on the farm of their master"; (2) "the domicile, hamlet, etc., of such slaves, their master dwelling in the city"; or, (3) more simply, a "slave village."[32] *Gandu*, on the other hand, is apparently an old Hausa word, probably borrowed from Songhai, the medieval empire that strongly influenced the economy of the Hausa region before the 1590s.[33] It has been defined as a

[31] Hill, "From Slavery to Freedom: The Case of Farm-Slavery in Nigerian Hausaland," *Comparative Studies in Society and History*, 18 (1976): 395–426. Also see Hill, "From Slavery to Freedom: The Case of Farm-Slavery in Kano Emirate," paper presented at the Kano Seminar, 1976, "Comparative West African Farm-Slavery Systems (South of the Sahel) with Special Reference to Muslim Kano Emirate (N. Nigeria)," paper presented at the Symposium on Islam in Africa: Slavery and Related Institutions, Princeton University, 1977, and *Population, Prosperity, and Poverty: Rural Kano, 1900 and 1970*, 206–16. For a further criticism of this approach, see Lovejoy, "Plantations in the Economy of the Sokoto Caliphate," 341–68.

[32] G. P. Bargery, *A Hausa-English Dictionary* (London, 1934), 854; and R. C. Abraham, *Dictionary of the Hausa Language* (London, 1962), 84.

[33] Neil Skinner, "The Origin of the Name 'Hausa,'" *Africa*, 38 (1968): 253–57; Smith, *The Economy of Hausa Communities of Zaria*, 19–21, 113; and Polly Hill, *Rural Hausa: A Village and a Setting* (Cambridge, 1972), 38–56, and *Population, Prosperity, and Poverty: Rural Kano, 1900 and 1970*, 205–06, 207. For a discussion of the influence of Songhai on the Hausa economy, see Paul E. Lovejoy, "The Role of the Wangara in the Economic Transformation of the Central Sudan in the Fifteenth and Sixteenth Centuries," *JAfH*, 19 (1978): 173–93. It should be noted, in support of this interpretation and as further evidence of links with the sixteenth-century Songhai economy, that the term for plantation in the Bariba states, which were located south of Songhai and west of

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"large farm," but in many cases the term was synonymous with *rinji*.[34] The current use of *gandu* indicates the cooperative exploitation of land and other economic resources by two or more, usually related, adult males on behalf of their wives and dependents, but, as M. G. Smith has shown,

> In the nineteenth century . . . the *gandu*, which was the principal type of organization for farming, included slaves and their descendants. . . . Sometimes the slaves' huts were situated within the same walled or fenced enclosure as those of the owners' family, though in a different section. Where holdings of slaves were large, the *gandu* ceased to be contained within a single compound, though it still functioned as a single unit for the production and consumption of food, the payment of tax, the provision of farm tools, and seed, the common exploitation of land, economic trees, and other resources. . . .[35]

The Fulfulbe term, *rumde*, which Abubakar has defined as an "agrarian slave settlement," was prevalent in Adamawa and wherever Fulfulbe was the major language. Dialectal variations included *roumdi*, *ruumd'en*, and *ruumdandi'en*. In Nupe *tunga* (pl. *tungazi*) was the equivalent.[36] The translation of *rinji*, *rumde*, and *tunga* as "slave village" and, more rarely, as "feudal estate" suggests some difficulties in equating this type of organization with plantations. The degree of market development and the nature of the master-slave relationship, however, were such as to make "feudal" inappropriate and "village" inadequate. Nonetheless, it is clear that large-scale agriculture existed; only the extent and organization of such practices is in question.

NUMEROUS, INDEPENDENT ESTIMATES on the proportion of slaves in caliphate society reveal that probably 50 percent or more of the population was servile,[37] although the percentage of slaves varied from emirate to emirate. By the end of the nineteenth century, for example, apparently most people in the Sokoto area were slaves, and this pattern extended to Gwandu, the twin capital of the caliphate.[38] The Kano, Zaria, Nupe, and Katsina emirates also had large numbers

the Hausa country, was *gando*; see Bernard Baldus, "Responses to Dependence in a Servile Group: The Machube of Northern Benin," in Miers and Kopytoff, *Slavery in Africa*, 438.

[34] Smith, "Slavery and Emancipation in Two Societies," 141; Mahdi Adamu, "The Delivery of Slaves from the Central Sudan to the Bight of Benin in the Eighteenth and Nineteenth Centuries," in Gemery and Hogendorn, *The Uncommon Market*, 163–80; Bargery, *A Hausa-English Dictionary*, 357; and Abraham, *Dictionary of the Hausa Language*, 296.

[35] Smith, "Introduction," 22. Also see Smith, "Hausa Inheritance and Succession," in J. D. M. Derrett, ed., *Studies in the Laws of Succession in Nigeria* (London, 1965), 241–45.

[36] Sa'ad Abubakar, *The Lāmībe of Fombina: A Political History of Adamawa, 1809–1901* (Zaria, 1979); D. J. Stenning, *Savannah Nomads* (Oxford, 1959), 17; and Mason, "Captive and Client Labour and the Economy of the Bida Emirate, 1857–1901," 453–71. For a discussion of practices elsewhere in West Africa, where similar terminology was used to describe this agricultural organization, see Johnson, "The Economic Foundations of an Islamic Theocracy: The Case of Masina," 488–89; Baldé, "L'esclavage et la guerre sainte au Fouta Jalon," 195–202; Patenostre, "La captivité chez les Peul du Fouta-Djallon," *Revue Outre-Mer*, 3 (1930): 243–54; and Réné Caillie, *Travels through Central Africa to Timbuctoo*, 1 (London, 1830): 173, 210–11.

[37] For a useful summary of nineteenth-century estimates, see Hill, *Rural Hausa: A Village and a Setting*, 40, 319. E. A. Ayandele has noted that "without slaves the economy would have collapsed"; see Ayandele, "Observations on Some Social and Economic Aspects of Slavery in Pre-Colonial Northern Nigeria," *Nigerian Journal of Economic and Social Studies*, 9 (1967): 333.

[38] F. D. Lugard, *Instructions to Political and Other Officers, on Subjects Chiefly Political and Administrative* [hereafter *Political Memoranda*] (London, 1906), 296, 301; and S. A. Balogun, "Economic Activities and Ties of Gwandu Emirates and Their Neighbors in the Nineteenth Century," paper presented at the Kano Seminar, 1976.

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of slaves, while there were heavy concentrations in Fombina (Adamawa), Ilorin, Liptako, and elsewhere.[39] These proportions not only demonstrate that large numbers of slaves were available for agricultural activities but also reflect the existence of a mechanism of labor supply.

Slaves were readily available in the nineteenth century, and their cost was low.[40] Caliphate military campaigns fed a steady stream of war captives into the economy, and this ready supply of slaves accounts for the relative stability of slave prices during the whole century, with real prices perhaps even declining slightly in the last third. The aristocracy retained many of these captives, but a well-developed commercial sector also transferred them from the front lines of battle to central markets. Indeed, merchants often invested in slaves for use in agriculture. Slaves, therefore, ended up in the countryside in one of two ways: some were redistributed among the military and aristocracy by the government and some were sold in such market places as those at Kano, Zaria, Katsina, and other major centers.

Individual slave holdings were often large. One emir of Zaria, Muhammad Sani (1846–60), had three thousand located at Taban Sani, while other Zaria officials reportedly held concentrations of slaves in excess of one thousand.[41] Imam Imoru, the learned scholar, writer, and jurist who wrote extensively on caliphate economy and society as he knew it at the end of the nineteenth century, reported that substantial slave holdings were common:

> There are slave owners in Hausaland who have purchased 100 and 200 slaves, and there are some slave owners with 400 or 500 slaves. But, in my lifetime, I have heard of only one person who was not an official, who had 1,000 slaves in Kano: his name was Kaushe. . . . In Hausaland, commoners buy slaves, but they are unable to buy 1,000 of them. Officials have more than 1,000 because they do not buy them: they seize them during war campaigns.[42]

Hanwa and Biye, two estates near Zaria city, had roughly forty and one hundred slaves respectively, while Karo, an estate near the Zaria-Kano border had close to two hundred and fifty. Some wealthy merchants in Kano owned hundreds of agricultural slaves. In late nineteenth-century Dorayi, near Kano, thirteen large slave owners held several hundred slaves among them, including the

[39] Hill, "From Slavery to Freedom: The Case of Farm-Slavery in Nigerian Hausaland," 395–426; Mason, "Captive and Client Labour and the Economy of the Bida Emirate, 1857–1901," 453–71; Allan Meyers, "Slavery in the Hausa-Fulani Emirates," in Daniel F. McCall and Norman R. Bennett, eds., *Aspects of West African Islam*, Boston University Papers on Africa, no. 5 (Boston, 1971), 173–84; Smith, *The Economy of Hausa Communities of Zaria*, 81–82, and "Slavery and Emancipation in Two Societies," 239–80; Lugard, *Political Memoranda*, 296–302; Irmgard Sellnow, "Die Stellung der Sklaven in der Hausa-Gesellschaft," *Mitteilungen aus den Institut für Orientforschung*, 10 (1946): 85–102; Irwin, "Visions of Times Past: The Oral Histories of Liptako"; and J. C. Froelich, "Le commandement et l'organisation sociale chez les Fulbe de l'Adamoua (Cameroun)," *Études Camerounaises*, 45–46 (1954): 5–91.

[40] Tambo, "Sokoto Caliphate Slave Trade in the Nineteenth Century," 187–217; Smaldone, *Warfare in the Sokoto Caliphate*, 13–14, 72, 134–37, 147–49; Adamu, "The Delivery of Slaves from the Central Sudan to the Bight of Benin," 163–80; Lovejoy and Hogendorn, "Slave Marketing in West Africa," 213–35; and J. S. Hogendorn, "Slave Acquisition and Delivery in Precolonial Hausaland," in R. Dumett and B. K. Schwartz, eds., *West African Culture Dynamics: Archaeological and Historical Perspectives* (Chicago, forthcoming).

[41] M. G. Smith, *Government in Zazzau* (London, 1960), 157.

[42] Douglas Edwin Ferguson, ed., "Nineteenth-Century Hausaland, Being a Description by Imam Imoru of the Land, Economy, and Society of His People" (Ph.D. dissertation, University of California at Los Angeles, 1973), 233.

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emir of Kano, who owned fifty. Likewise in Katsina, Adamawa, Nupe, Daura, Liptako, Bauchi, and other emirates, aristocrats and commoners alike amassed comparable numbers of slaves, ranging from a few score to several hundred and even several thousand.[43]

The caliphate government controlled land allocation and actively promoted agricultural development through the free distribution of fields to slave owners. Usually, immigrant merchants and craftsmen applied to local officials for a land grant, and upon payment of a small gift as *gaisuwa* (greeting or homage) an area of uncleared bush was set aside. Only improved land, especially low-lying tracts that could be irrigated (*fadama*) but also manured farms, were saleable, although sales may have been rare where unused land was plentiful. In Zaria, and probably elsewhere as well, individuals were not permitted to establish separate, walled communities without specific government approval, but anyone with slaves could obtain enough land to start a plantation.[44] Although plantations usually belonged to individuals, some were owned by dynastic families and less frequently were attached to particular offices.

The crops grown on these plantations included grain, particularly bullrush millet and sorghum, cotton, indigo, tobacco, locust beans, shea nuts, kola nuts, cow peas, peanuts, rice, and sugar cane. Regional specialization was a significant dimension of production. Kola nuts were grown only in Nupe; cotton and indigo were most extensively cultivated in Zamfara, southern Katsina, northern Zaria, and Kano, although both were grown to some degree elsewhere. Tobacco and shea nuts were cash crops everywhere, but tobacco may have been a major plantation crop only in Katsina and parts of Kano, and shea nuts became most important in the Nupe economy at the end of the nineteenth century. Crops could be distributed over a distance of several hundred kilometers, particularly along the desert edge and within the textile belt of southern Kano and northern Zaria. Grain, for example, was carried north into the sahel and southern Sahara for sale to livestock breeders and salt miners, although because of fluctuations in

[43] Hogendorn, "The Economics of Slave Use on Two 'Plantations' in the Zaria Emirate," 372; Lovejoy, "Plantations in the Economy of the Sokoto Caliphate," 346–48, and *Caravans of Kola: Hausa Trade with Asante, 1700–1900*, chap. 5; Smith, *Baba of Karo*, 37–40; M. G. Smith, *The Affairs of Daura: History and Change in a Hausa State, 1800–1958* (Berkeley and Los Angeles, 1978), 32, 43, 268–69, 271, 277–78, 281, "Development and Organization of the Fulani Chiefdom of Kaura Namoda, 1809–1903," Papers of the Institute of Administration, Ahmadu Bello University, Zaria (1972), 49–50, and "Slavery and Emanicpation in Two Societies," 116–61; Hill, "From Slavery to Freedom: The Case of Farm-Slavery in Nigerian Hausaland," 417–20; Usman, "Transformation of Katsina, c. 1796–1903"; Mason, "Captive and Client Labour and the Economy of the Bida Emirate, 1857–1901," 453–71; Abubakar, "A Survey of the Economy of the Eastern Emirates of the Sokoto Caliphate," and *Lamibe of Fombina*; R. J. Gavin, "The Economy of Ilorin," paper presented at the University of Birmingham, 1976; Heinrich Barth, *Travels and Discoveries in North and Central Africa*, 2 (New York, 1859): 159, 163, 174, 190–96; Lugard, *Political Memoranda*, 296–302; Froelich, "Le commandement et l'organisation sociale chez les Fulbe de l'Adamoua," 5–91; Siegfried Passarge, *Adamaua: Bericht über die Expedition des deutschen Kamerun-Komitees in den Jahren 1893/94* (Berlin, 1895), 108, 261–62; R. Bruce, "The Gindiri Rinji, 1890–1907" (forthcoming); and David C. Tambo, "The Pre-Colonial Tin Industry in Northern Nigeria" (forthcoming). Extensive data on the plantation system are also contained in the oral testimonies collected during the Economic History Project, Ahmadu Bello University, Zaria, 1975–76, and deposited there. Relevant collections include those by J. S. Hogendorn (Zaria), P. E. Lovejoy and A. Maccido (Zaria), Aliyu Bala Umar (Kano), Yusufu Yunusa (Kano), O. S. Ahmed (Nassarawa), Aliyu Musa (Kano), A. Babangida (Hadejia), and Musa Ahmed (Katagum). For a discussion of these, see P. E. Lovejoy and J. S. Hogendorn, "Oral Data Collection and the Economic History of the Central Savanna," *Savanna*, 7 (1978): 71–74.

[44] Smith, *Government in Zazzau*, 81.

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rainfall, distribution varied from year to year within the sahel. Cotton and, to a lesser extent, indigo were taken to the numerous dye works of the textile belt, and some cotton at least was carried great distances, even into the sahel, where it was spun and woven for re-export south to be dyed.[45] Besides the bulk movement of these commodities and the transport of produce from countryside to city, a few items were exported beyond the borders of the caliphate. These included dried onion leaves, tobacco, and prepared indigo. Indirectly, of course, the production of the textile industry also accounted for the export of agricultural commodities, although in the form of finished goods. Plantations produced a variety of other crops, often consumed on the plantation or in the master's household but sometimes sold on the market. At Hanwa and Biye, two Zaria plantations analyzed by Hogendorn,

> All the guinea corn and millet produced on the *gandu* fields was transported to *ubangiji*'s [the master's] granaries. These stocks were the source of the slaves' noontime meal and the evening meal of the unmarried slaves. They were also the staple in *ubangiji*'s household, whether at the *rinji* or at his town quarters in Zaria. Little guinea corn or millet was sold, although a large proportion of the *gandu* output of rice, cassava, potatoes, and cowpeas, as well as cattle and milk, went to market. The proceeds from the sale of crops, livestock, and livestock products were the source of the cowries needed for the *rinji*'s annual tax bill.[46]

Elsewhere, the proportion and types of crops destined for the market varied, but usually produce raised on irrigated farms was sold, as was grain in times of surplus.

Beyond these general conditions of caliphate agriculture, some specific similarities to plantations in the Americas are strikingly apparent. M. G. Smith, who based his analysis on a comparison of Zaria *rinji* with Jamaican plantations, summarized these as follows:

> (1) There was a division of labor time for the slaves between their master's plantation, and their own gardens. (2) Food, separate housing, tools, certain allowances of clothing and farm-plots were allotted to slaves, and with the exception of these allocations they were held responsible for their own maintenance. (3) They were supervised directly by slave headmen or officials who were then responsible to free persons with managerial functions. (4) . . . slave estates were economic and social units in which there was little place for strangers. (5) The principal occupation was agriculture. (6) There was a division between field gangs and slaves engaged in domestic work . . . , the principal domestic tasks . . . being the preparation of food in bulk for the slaves, and the care of the owner's household.[47]

[45] For a discussion of the development of the regional market, see Lovejoy, "Plantations in the Economy of the Sokoto Caliphate," 342–68, "Interregional Monetary Flows in the Precolonial Trade of Nigeria," 563–85, "The Borno Salt Industry," *International Journal of African Historical Studies*, 10 (1977): 465–80, and *Caravans of Kola: Hausa Trade with Asante, 1700–1900*, chaps. 3–5; Lovejoy and Baier, "The Desert-Side Economy of the Central Sudan," 551–81; Philip Shea, "The Development of an Export-Oriented Dyed Cloth Industry in Kano Emirate in the Nineteenth Century" (Ph.D. dissertation, University of Wisconsin, 1975); Abubakar, "A Survey of the Economy of the Eastern Emirates of the Sokoto Caliphate"; and Mason, "Trade and State in Nineteenth-Century Nupe," and "Captive and Client Labour and the Economy of the Bida Emirate, 1857–1901," 453–71.

[46] Hogendorn, "The Economics of Slave Use on Two 'Plantations' in the Zaria Emirate," 377.

[47] Smith, "Slavery and Emancipation in Two Societies," 141.

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If various parts of the caliphate are examined with reference to Smith's criteria, then it becomes clear that these conditions were widespread.

The division of land holdings between *gandu* fields and *gayauna*, or private plots farmed by individual slaves and their immediate families, was common everywhere. This is particularly clear in early colonial reports, specifically in the sources for F. D. Lugard's 1906 memorandum on slavery. Thus, in Bauchi "a farm slave is given a separate plot of land for his own use, and is allowed to sell the produce of it, after his master has sold his own."[48] Similarly, a description of an estate at Magaria, near Sokoto, in 1824 makes it clear that cultivated land was divided between the main plantation field and smaller plots for the slaves:

> When the time for cultivating the ground and sowing the seed comes on, the owner points out what he requires, and what is to be sown on it. The slave is then allowed to enclose a part for himself and family. The hours of labor, for his master, are from daylight until mid-day; the remainder of the day is employed on his own, or in any other way he may think proper. At the time of harvest, when they cut and tie up the grain, each slave gets a bundle of the different sorts of grain, about a bushel of our measure, for himself. The grain on his own ground is entirely left for his own use, and he may dispose of it as he thinks proper.[49]

Only Mason has described situations in which there do not appear to have been plantation fields where large concentrations of slaves worked. He has reported that slaves in Nupe "on *tungazi* did not divide their day between working for their masters and for themselves as seems to have been the case in other parts of West Africa. They were normally left to themselves under a village head (*et-suemkpa*) of slave or non-slave origin, and remitted farm produce to their masters on demand or at customary times such as harvest."[50] It is likely that Mason is describing situations in which *murgu* payments were involved, whereby slaves, frequently second or subsequent generations, were permitted to hire themselves out for a fixed amount, a practice often related to self-redemption. Farming on one's own was one method that slaves pursued in order to make these payments, and it was indeed a very significant dimension of the slavery system.[51]

Besides granting *gayauna* plots, plantation owners were expected to provide accommodation, food during breaks from *gandu* work, tools that could also be used in private gardens, and minimal amounts of clothing.[52] At Nassarawa in 1905, for example,

> a serf receives a house and farm for himself, which he can work on two days in the week, and on any other days if he has time to spare, but his master can claim all his time on

[48] Lugard, *Political Memoranda*, 302–03.

[49] H. Clapperton, *Journal of a Second Expedition into the Interior of Africa* (London, 1829), 214.

[50] Mason, "Captive and Client Labour and the Economy of the Bida Emirate, 1857–1901," 465–66.

[51] Lugard, *Political Memoranda*, 300. For practices elsewhere, see Hill, "From Slavery to Freedom: The Case of Farm-Slavery in Nigerian Hausaland," 418, and *Rural Hausa: A Village and a Setting*, 299; Hogendorn, "The Economics of Slave Use on Two 'Plantations' in the Zaria Emirate," 374–75; Bruce, "The Gindiri Rinji, 1890–1907"; and various oral testimonies in the Economic History Project.

[52] Polly Hill has offered a useful summary of available material on the obligations of *gandu* heads to their dependents, although she apparently is unaware of the significance of her analysis for an understanding of the plantation sector; see *Rural Hausa: A Village and a Setting*, 38–56, and *Population, Prosperity, and Poverty: Rural Kano, 1900 and 1970*, 204–05. Also see Smith, *The Economy of Hausa Communities of Zaria*, 19–21, "Hausa Inheritance and Succession," 241–45, and *The Affairs of Daura: History and Change in a Hausa State, 1800–1958*, 48; and Hill, "From Slavery to Freedom: The Case of Farm-Slavery in Nigerian Hausaland," 400–02.

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the other five days. If the produce of his farm is not enough, the master must contribute to his support. The slave must keep his house in repair, and do any building work required. The master supplies clothes and tools, also incidentally a wife. In case of sickness or absence, the burden of the support of the family lies on the master.[53]

This account draws attention to another significant obligation of plantation owners—the provision of spouses for their slaves. In using "serf" to describe plantation inhabitants, some colonial officials attempted to divert attention from the prevalence of slavery.

Overseers were commonly appointed to run plantations. They were called *sarkin gandu*, and sometimes only *gandu*, to add to the confusion over the use of that term. Occasionally, other titles were employed, and in the case of especially large and dispersed holdings, such as those of the emirs of Kano and Zaria, overseers were responsible to a manager who lived in the master's household. For the emir of Kano this official was the *shamaki*, and both he and the overseers were slaves by origin, although their authority and responsibilities placed them in a far different status category from that of field hands. The use of subordinates to manage plantations was related to a high degree of absenteeism among the largest owners. Often masters lived in emirate capitals or towns and only occasionally visited the plantations. Government officials, military personnel, and wealthy merchants were engaged in other activities that prevented them from living on their estates.[54]

Slaves usually had separate quarters, but sometimes they lived in separate villages without freemen. The 250 slaves on the Karo estate of northern Zaria, for example, lived apart from the master's family which also maintained residences in nearby Zarewa town, and for a number of years in Zaria city as well.[55] Sometimes slaves were kept in the urban homes of masters, but they inhabited the outer parts of the compound.[56] It may be that many towns founded in the nineteenth century grew up around an initial, large plantation. The process of urbanization intensified in southern Kano and northern Zaria in the late nineteenth century because of the raids of Ningi renegades, and freemen and slaves alike moved into walled settlements to seek protection. Even when slaves were left on the plantations, as at Karo, they evacuated the estate if raiders were sighted.[57]

While agriculture was the dominant activity in the many communities that

[53] Lugard, *Political Memoranda*, 301–02.

[54] Hogendorn, "The Economics of Slave Use on Two 'Plantations' in the Zaria Emirate," 374; and the Yusufu Yunusa Collection (Kano) and the Lovejoy-Maccido Collection (Zaria), Economic History Project. Reports of slaves selecting their own overseers exist for some areas, but the accuracy of these observations is not clear; see Lugard, *Political Memoranda*, 301.

[55] Smith, *Baba of Karo*, 39. Also see Hogendorn, "The Economics of Slave Use on Two 'Plantations' of the Zaria Emirate," 377; and Hill, "From Slavery to Freedom: The Case of Farm-Slavery in Nigerian Hausaland," 418.

[56] Musa na Madabo, Tape 14, Lovejoy Collection, 1969–70, Graduate Reading Room, Department of History, Ahmadu Bello University; and numerous interviews in the Yusufu Yunusa (Kano), Aliyu Bala Umar (Kano), and Lovejoy-Maccido (Zaria) Collections, Economic History Project.

[57] For examples of resistance to caliphate rule, see S. A. Balogun, "The Place of Argungu in Gwandu History," *Journal of the Historical Society of Nigeria*, 7 (1974): 403–15; Adell Patton, Jr., "Frontier 'Losers' in the Ningi Rebellion: A Neglected Theme in Nineteenth-Century Rural Nigerian Hausaland," paper presented at the annual meeting of the African Studies Association, Baltimore, 1978; Smith, *Baba of Karo*, 38–39, 46–47, 66–71; and the Hogendorn (Zaria) and Lovejoy–Maccido (Zaria) Collections, Economic History Project.

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are described here as plantations, other duties were also important. Dry-season activities included house repair and construction, cutting grass and sticks for these tasks, collecting firewood, gathering other sylvan produce, digging wells, carrying crops to market or to storage bins in urban households, and transporting manure and compound sweepings to the farms. All were related to plantation management, and farming frequently continued into the dry season, since cotton harvests were much later than those for grain, and irrigated lowlands (*fadama*) were planted with such crops as sugar cane, onions, wheat, indigo, and other produce. The amount of time slaves had to work on their own varied. In some places only one day off was allowed, while in other places two days was the practice. Slaves were also permitted to work on their own in the late afternoons.[58]

The division of labor between fieldwork and domestic chores was widespread. Both men and women worked in the fields, but some women at least assisted in preparing food for laborers and remained in the compound under the supervision of the wives of the master or overseer. The division between field hands and house servants was most pronounced in the distinction between the palace slaves of the aristocracy and their agricultural workers in the countryside.[59] Sometimes, however, slaves may have worked under the *murgu* system, whereby a fixed amount of money was remitted to the master. In this case, production units more closely corresponded to the nuclear family, and, therefore, gang labor did not occur.[60]

DESPITE THE MANY SIMILARITIES with plantation economies elsewhere, there were significant differences, which suggest that caliphate plantations were unique. These differences relate to the greater degree of market development in the Americas and along the East African coast in the nineteenth century. Distinctions were important in these other plantation areas, even within individual countries, but the caliphate was even more distinct.[61] In nineteenth-century Brazil, for example, slave practices in the coffee-growing regions of the south were much harsher than in the more paternalistic slave society of the sugar-producing northeast. And the plantations in Zanzibar and Pemba were strongly influenced by the mediating role of Islamic norms, despite the pull of market demand for cloves and other products. Plantation slavery in the Sokoto Caliphate appears to have been several stages beyond the relative leniency of either north-

[58] Stenning, *Savannah Nomads*, 17; Bruce, "The Gindiri Rinji, 1890–1907"; Hogendorn, "The Economics of Slave Use on Two 'Plantations' of the Zaria Emirate," 375, 381; Lugard, *Political Memoranda*, 301; and the Lovejoy-Maccido (Zaria) and Yusufu Yunusa (Kano) Collections, Economic History Project.

[59] For a discussion of the sexual division of labor on the Karo estate, see Smith, *Baba of Karo*, 38.

[60] Mason, "Captive and Client Labour and the Economy of the Bida Emirate, 1857–1901," 465–66; Ferguson, "Nineteenth-Century Hausaland, Being a Description by Imam Imoru," 230; and Lugard, *Political Memoranda*, 300.

[61] For a general comparison of African and American slavery, see Cooper, "The Problem of Slavery in African Studies," 103–25; and Smith, "Slavery and Emancipation in Two Societies," 116–61. Also see Eugene D. Genovese, ed., *The Slave Economies*, 2 vols. (New York, 1973); and Laura Foner and Eugene D. Genovese, eds., *Slavery in the New World* (Englewood Cliffs, N.J., 1969). In the following discussion of the differences from American patterns, I have benefited greatly from conversations with Marc Egnal of York University.

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east Brazil or East Africa in terms of the rights and privileges of slaves. This indicator that market forces were weaker in the caliphate suggests that, where such conditions prevailed, familial and traditional bonds tended to assert themselves in shaping slave society. When a slaveholder's income was limited, he was forced to allow the slave other sources of income and greater participation in society. Plantation slaves did not have political influence, but they could expect to marry, to be accepted as Muslims, and to look forward to the possibility of earning or otherwise acquiring their freedom. Such leniency does not mean that some slaves were not treated harshly; it only means that the pattern of plantation slavery fit into a slave system that was generally less restrictive than that in the Americas.

The extent of internal trade in the Sokoto Caliphate was relatively limited, and hence the basis of a dynamic slave economy was restricted. Grain was a chief crop, which is exceptional for plantation agriculture. In part this was related to the existence of the desert-side sector, a market without parallel in the Americas or East Africa. Grain demand could not expand beyond a quite low level, for the desert edge could support only a limited population and grain production in the sahel could satisfy much of the local need, except in bad years. Although transport costs might appear to inhibit market development, in fact the movements of transhumant pastoralists meant that such costs were very low.[62] Within the savanna, grain also was distributed among regions, perhaps most notably into the textile manufacturing belt in southern Kano and northern Zaria, where it was more profitable to grow other crops. Again, transhumance accounts for the relatively low cost of transport, but, even so, the market was never sufficient to encourage slaveholders to produce just for sale.[63] Surplus was disposed of, but most planters raised other crops specifically for market exchange, particularly irrigated crops such as wheat, onions, tobacco, indigo, and cassava, and these were primarily intended for regional distribution. This lack of a major cash crop for export is a striking indication of the minimal level of market development and opportunity.

In some areas, cotton and indigo became cash crops almost comparable to the specialized production that characterized agriculture in the American tropics and the East African coast. This specialization was especially prevalent in the area of textile concentration south of Kano city and in a belt to the east, south, and west of this core.[64] Again, however, the extent of market orientation was restricted, and the lack of economies of scale meant that independent peasants, often with a few slaves as field hands, competed for the same market. Indeed, indigo was often a speciality of Maguzawa peasants, who produced indigo under cooperative arrangements in order to pay the tax incumbent on non-Muslim

[62] Lovejoy and Baier, "The Desert-Side Economy of the Central Sudan," 557; and Stephen Baier, "Local Transport in the Economy of the Central Sudan, 1900–1930," paper presented at the Kano Seminar, 1976, and "Trans-Saharan Trade and the Sahel: Damerghou, 1870–1930," *JAfH*, 18 (1977): 37–60. Grain was also an important crop in East Africa; see Cooper, *Plantation Slavery on the East Coast of Africa*, 80–86.

[63] Lovejoy, "Plantations in the Economy of the Sokoto Caliphate," 346–47, 354, 357–58.

[64] *Ibid.*, 355–59; and Shea, "The Development of an Export-Oriented Dyed Cloth Industry in Kano Emirate," 53–140.

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populations. Significantly, this communal farming was also called *gandu* and suggests the adaptability of that term.[65] The type of cotton also demonstrates the difference from cash cropping in the Americas. It grew on bushes which produced for several years, although with diminishing returns. Though cotton production was lower in cost than in the Americas, it was still high in cost by the standards of other crops in the caliphate. Nonetheless, labor requirements, despite much lower yields than American varieties, were relatively lower during nonplanting years.

Another difference related to the production of cotton and indigo for the domestic market, not for direct export.[66] Finished textiles were the major export from the caliphate, and there was also a large internal demand. Cotton, however, was not an export crop, except for a limited extent to the desert edge, where it was carded, spun, and woven into narrow strips for reshipment back to the dyeing centers of the savanna. Indigo was exported in small quantities; the Kano variety was particularly well regarded as a dye and commanded a small market. Hence, cotton and indigo did supply an export trade, but the crops themselves were primarily consumed domestically. Production was for a regional market that had an export sector, but this regional market contrasts sharply with the usual situation for highly capitalized plantation economies, where export markets are the principal destinations for the cash crop. Clearly, transport was the limiting factor here. Costs within transhumant corridors could facilitate exchange, but, outside of these desert-side patterns, expenses were too high to enable market growth beyond a fairly low level.

The lack of staple crops, except in quite limited areas, meant that most planters acquired relatively little disposable income from the sale of produce. Some money was necessary to pay taxes and purchase luxuries and necessities, but these expenses were met through the sale of surplus grain, cotton, indigo, or irrigated crops.[67] All of the available evidence indicates that the low level of technology, particularly in transport but also in textile production, prevented crop specialization beyond a relatively local market. Since taxes were also paid in kind, moreover, most output was destined for subsistence consumption, either within the plantation or in its immediate neighborhood.

If the economic basis of a slave system that was comparable to the Americas and the East African coast was lacking, a more detailed examination of the plantations themselves suggests that they, too, were different. The frequent identification of *rinji, tungazi,* and *rumde* with villages is one significant indication, especially in cases where overseers were slaves who acted as much like village headmen as organizers of gang labor. In addition, many slaveholdings were fragmented, a tendency that emancipation and inheritance practices encouraged. My earlier research on the investments of kola traders in the Kano area demonstrates that, even when merchants owned several hundred slaves,

[65] Hill, *Rural Hausa: A Village and a Setting*, 39. It should be noted that the existence of gigantic households, analyzed by Hill, may also reflect the restricted pull of economic forces; *ibid.*, 56.

[66] Shea, "The Development of an Export-Oriented Dyed Cloth Industry in Kano Emirate," 53–140; and Lovejoy, "Plantations in the Economy of the Sokoto Caliphate," 355–59.

[67] Hogendorn, "The Economics of Slave Use on Two 'Plantations' in the Zaria Emirate," 377.

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landholdings were often scattered.[68] Smaller farms, called *gona*, were distinguished from *gandu* or *rinji*, and the combined acreage of dispersed farms could exceed the area of a single plantation. In the case of *gona*, slaves were sent from one location to another, and they usually lived in the urban compound of the master.

The management practices of wealthy Agalawa and Tokarawa merchants demonstrate this pattern of dispersed holdings. The Agalawa and Tokarawa lived in Kano and Katsina Emirates and were involved in exporting textiles, salt, leather goods, and other commodities to Asante in exchange for kola nuts, a widely used stimulant in Muslim society. The Agalawa, Tokarawa, and other groups of merchants identified as Hausa commoners; and, because of their success, they were widely respected as an example of the social mobility achieved through caravan trading and plantation investment.[69] The fields of Tambarin Agalawa Yakubu, the wealthy kola importer who died in the early 1890s, for example, included Makafi, Gonar Kuka, Allah Tayamu, and Gonar K'ofa within the walls of Kano city, while he owned three other small farms, Kankara, Gonar Kurna, and Gonar Tinshe, outside the walls. In addition to these, he had two larger farms and a *gandu* at Rijiyar Lemo, just west of the city. Finally, he managed farms at Rantan, near Kura, and probably at several other villages as well. Family tradition credits Yakubu with as many as five hundred slaves, and it certainly seems likely that he owned several hundred in order to farm these lands.[70] It is significant, however, that only one of Yakubu's holdings was called a *gandu*. Other merchant-planters followed this pattern. One Agalawa trader who lived in Kano city had farms at Shibdawa in Katsina Emirate, others at Lambu, Garim Malam, and Chiroma in Kano Emirate, and a number of gardens within Kano's walls. Similarly Madugu K'osai, the famous late nineteenth-century Tokarawa caravan leader, owned Kanyan Amana, near Fanisau, north of Kano city, on which he kept sixty slaves, and he had farms at Damargu, near Bichi in Kano Emirate, as well.[71]

Even in cases where slaves farmed a single agricultural unit, slave ownership was sometimes fragmented. At Dorayi, near Kano city, for example, a planter who was the village head at the end of the nineteenth century controlled the labor of about twenty slaves, but his wives owned some of these.[72] Of the two hundred and fifty slaves on the Karo plantation along the Kano-Zaria border, about one hundred and fifty were owned by Ibrahim Dara, the founder of the plantation. His relatives owned the rest, with one brother possessing forty.[73] Nonetheless, the plantation was managed as a single unit until Dara died, whereupon many slaves escaped and the rest were divided among the owners

[68] Lovejoy, *Caravans of Kola: Hausa Trade with Asante, 1700–1900*, chap. 5.

[69] Lovejoy, "The Kambarin Beriberi: The Formation of a Specialized Group of Hausa Kola Traders in the Nineteenth Century," *JAfH*, 14 (1973): 633–51, and *Caravans of Kola: Hausa Trade with Asante, 1700–1900*; and Lovejoy and Baier, "The Desert-Side Economy of the Central Sudan," 551–81.

[70] Musa na Madabo, Tape 14, Lovejoy Collection, 1969–70.

[71] Muhammadu Kasori, Tape 14, Lovejoy Collection, 1969–70; and Miko Hamshak'i, Bako Madigawa, and Audu Ba'are, Tape 12, Lovejoy Collection, 1969–70.

[72] Hill, "From Slavery to Freedom: The Case of Farm-Slavery in Nigerian Hausaland," 417–20.

[73] Smith, *Baba of Karo*, 37–40.

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and heirs for independent farming. Both fragmented ownership and the lack of centralized landholdings demonstrate the limited degree of economic rationalization. This was not an organization that suggests efficient, large-scale production for the market. Moreover, slaves had substantially more time to themselves, often a day or two per week plus late afternoons, so that they had relatively more opportunity for private production than the plantation economies of the Americas and East Africa allowed.

The slave trade also reflected an economy that was based on limited market development.[74] Rather than being freely bought and sold, many plantation slaves, perhaps even a majority, were seized in raids and warfare. Their redistribution, without resort to the market, favored the aristocracy and its supporters. These nonmarket sources of labor permitted agricultural investment by the aristocracy. Tribute into the central government included large numbers of slaves, for most captives obtained in annual raids were either passed on to higher authorities or parceled out as rewards for service. Slaves could and often did enter the market place, sometimes for export and sometimes for purchase by local inhabitants for their private use. But the market supplemented the redistributive system; it was not the sole means of acquiring a bonded labor force, as it was in the Americas and on the East African coast.

Finally, the issue of emancipation most dramatically distinguishes this slave economy, a feature that reflects the Islamic dimension of caliphate society as well as the limited influence of market forces. Emancipation in the Sokoto Caliphate was relatively easy and was widespread. In the American South, especially in the decades of the cotton boom, emancipation even for those who wanted to undertake it through the provisions of the personal liberty laws was exceedingly difficult, and the situation was not very different elsewhere in the Americas. *Murgu* arrangements, whereby trusted slaves could pay off their sale price over a specified period, *fansa*, or ransoming of relatives, and pious acts were accepted practice and openly encouraged by the ethos of caliphate society.[75] In this case, practice was somewhat similar to that in the plantation society of the East African coast, where Islam also promoted avenues of upward mobility for slaves.[76] In the caliphate, however, the tendency was far greater. An examination of the activities open to slaves, both during the process of self-purchase and after emancipation, reveals that important skills, including weaving, tailoring, and retail trading, were available. This indicates greater opportunities in acquiring skills than was usually the case in slave societies. When combined with

[74] Tambo, "Sokoto Caliphate Slave Trade in the Nineteenth Century," 187–217; Hogendorn, "Slave Acquisition and Delivery in Precolonial Hausaland"; and Adamu, "The Delivery of Slaves from the Central Sudan to the Bight of Benin," 166–78.

[75] Smith, "Hausa Inheritance and Succession," 245, 263, and *The Economy of Hausa Communities of Zaria*, 103; Hill, "From Slavery to Freedom: The Case of Farm-Slavery in Nigerian Hausaland," 399–400, and *Population, Prosperity, and Poverty: Rural Kano, 1900 and 1970*, 204; and F. D. Lugard, *Political Memoranda: Revision of Instructions to Political Officers on Subjects Chiefly Political and Administrative*, ed. A. H. M. Kirk-Greene (3d ed., London, 1970), 229, 232–34, 247–48.

[76] For an excellent discussion of emancipation in an Islamic society, see Cooper, *Plantation Slavery on the East Coast of Africa*, 242–52.

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the relative unimportance of race as a means of controlling slaves, the institution of slavery itself presents a unique situation.[77]

What seems most striking about plantations in the economy of the Sokoto Caliphate is not their existence but their peculiar nature. The slaves were owned, but they were not driven to the same extent as those in the Americas and East Africa. The master could demand only a portion of their labor because he could provide them only with part of their upkeep. In balance the slave developed skills and his own private economy, which made him much closer to peasant farmers than was the case elsewhere. Market forces did not encourage extreme exploitation of slaves. By allowing them to produce crops on their own account and to engage in craft production and retail trading, the masters propelled the slaves into the market. This reduced obligations because the slaves became responsible for part of their upkeep, and it provided the master with a dividend in the form of a cash payment by the slave. Market forces, even though they were relatively weak, still influenced the organization of productive factors.

Nonetheless, nonmarket principles of redistribution also characterized the Central Sudan economy. Slaves were available for purchase, but many ended up on rural estates without entering the market. Land was sold on occasion, but usually it was redistributed without resort to sale. The lack of a major cash crop for export was another feature that demonstrated an internal orientation and reflected the marginal nature of the market. Even the planter class differed; it was indigenous not foreign, and most capital was generated internally and did not come from outside the region.

Yet large-scale enterprise was an important dimension of caliphate agriculture, and hence the caliphate conformed to wider patterns of intensified slave use within Africa. Indeed, the use of slaves on plantations or in large slave villages was common in many non-Islamic areas in the nineteenth century, including Dahomey, Warri, the northern Igbo country, and Ibadan.[78] Elsewhere in Africa similar agricultural practices were also found, including in the areas around the Lunda capital in what is now Zaire, along trade routes into the interior of Bantu Africa from both the Indian Ocean and the Gulf of Guinea, and

[77] For a discussion of Fulbe attitudes toward slaves, see Baldus, "Responses to Dependence in a Servile Group: The Machube of Northern Benin," 437–56; M. Hiskett, "Enslavement, Slavery, and Attitudes towards the Legally Enslavable in Hausa Islamic Literature," paper delivered at the Conference on Islam in Africa: Slavery and Related Institutions, Princeton University, 1977; Irwin, "Visions of Times Past: The Oral Histories of Liptako"; and Smith, "Slavery and Emancipation in Two Societies," 116–61.

[78] For a disucssion of the plantation sector in non-Muslim areas of nineteenth-century West Africa, see Lovejoy, "Indigenous African Slavery"; Klein and Lovejoy, "Slavery in West Africa," 197–98, 205–06; Catherine Coquery-Vidrovitch, "De la traite des esclaves à l'exportation de l'huile de palme et des palmistes au Dahomey: XIX^{e} siècle," in C. Meillassoux, ed., *The Development of Indigenous Trade and Markets in West Africa* (London, 1971), 118–19; P. C. Lloyd, "The Itsekiri in the Nineteenth Century: An Outline Social History," *JAfH*, 4 (1963): 219; David Northrup, "Nineteenth-Century Patterns of Slavery and Economic Change in Southeastern Nigeria," *International Journal of African Historical Studies*, 12 (1979): 91–95; A. E. Oroge, "The Institution of Slavery in Yorubaland with Particular Reference to the Nineteenth Century" (Ph.D. dissertation, University of Birmingham, 1971), 158–211; and Robin Law, "Royal Monopoly and Private Enterprise in the Atlantic Trade: The Case of Dahomey," *JAfH*, 18 (1977): 573–76. Also see H. A. Gemery and J. S. Hogendorn, "Comparative Disadvantage: The Case of Sugar Cultivation in West Africa," *Journal of Interdisciplinary History*, 9 (1979): 431–32.

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near European footholds at Cape Town and Luanda. As David Birmingham has shown for Angola, the Portuguese established a few dozen coffee farms and plantations in the interior of Luanda in the late nineteenth century, and there had long been plantations on the island of Saõ Tomé.[79] These plantations appear to have been as different from those of the Islamic savanna as the ones in the Sokoto Caliphate were from those in the Americas.

All of these cases suggest that the development of large-scale agriculture employing slave labor requires more detailed attention in order to determine specific characteristics and to explore possible structural links. Plantations and villages with similarities to plantations became important at a time when plantation agriculture in the Americas was being transformed as a result of the abolition of the slave trade and the emancipation of slaves. The timing of these developments suggests an interconnection between various parts of the tropics, despite the uniqueness of the various African settings and the relative importance of the nonmarket mode of distribution there. The similarities are still significant enough to warrant the use of the same term for both areas, at least in many cases. It may be, as Cooper has suggested, that a distinction must be made between plantations and slave villages, depending upon the rights and obligations of slaves,[80] but until now the use of "slave village" has disguised many of the key issues.

As the case of the Sokoto Caliphate demonstrates, large-scale agricultural organization in the Islamic societies of the northern savanna was sufficiently similar to the plantations of the Americas, despite local variations, that this practice can be considered a type of plantation agriculture. The spread of these Islamic plantations was related to larger developments, including the transition from slave raiding beyond the periphery of the capitalist economic system to integration into it. Plantation development in the northern savanna balanced the shift to "legitimate" trade along the African coast. In both the coastal region and the Islamic interior, increased use of slaves was a product of slave supply. Sustained European and North African demand for slaves promoted the continuation of enslavement within West Africa well into the nineteenth century, and the institutions of enslavement did not collapse immediately when Europeans stopped buying slaves. Instead, supplies were maintained, and the plantation sector continued to expand until European colonialism accelerated the process of change by ending slave raiding, abolishing the slave trade, and promoting new forms of labor organization.

[79] J. Hoover, "The Seduction of Ruwej: Reconstructing Ruund History (the Nuclear Lunda: Zaire, Angola, Zambia)" (Ph.D. dissertation, Yale University, 1978), 99–100 (I wish to thank Joseph Miller for this reference); David Birmingham, "The Coffee Barons of Cazengo," *JAfH*, 19 (1978): 528–29, 535, 538; and Lovejoy, "Indigenous African Slavery."

[80] Cooper, "The Problem of Slavery in African Studies," 112–17.

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AHR Forum

Social History as Lived and Written

JAMES A. HENRETTA

THERE ARE MANY PRACTITIONERS of the "new social history" in the United States, but few theorists or philosophers. No manifesto marked its advent, and no single handbook or work of scholarship decisively shaped its development. A few prescient observers—William O. Aydelotte, Lee Benson, Jesse Lemisch, and Robert F. Berkhofer, Jr., among others—have provided the new movement with some form and direction, but theoretical writings have been less important than substantive works of historical analysis.[1] Instruction has been by example, not by precept. Between 1961 and 1965 a number of European and American scholars published seminal works of social history, and graduate students and established academics in the United States quickly utilized the new insights and innovations in their own research.[2] The results are diverse and uneven, in part because of the decentralized institutional structure of American historical scholarship. Two trends are, however, readily apparent: (1) causal precision in the methods of quantitative research and (2) analytic expertise in the theories of the social sciences.

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[1] Aydelotte, "Quantification in History," *AHR*, 71 (1965–66): 803–25; Benson, *Toward the Scientific Study of History: Selected Essays of Lee Benson* (Philadelphia, 1972); Berkhofer, *A Behavioral Approach to Historical Analysis* (New York, 1969); and Lemisch, "The American Revolution Seen from the Bottom Up," in Barton J. Bernstein, ed., *Towards a New Past: Dissenting Essays in American History* (New York, 1968), 3–45.

[2] Philippe Ariès, *Centuries of Childhood: A Social History of Family Life*, trans. Robert Baldick (London, 1962); Lee Benson, *The Concept of Jacksonian Democracy: New York as a Test Case* (Princeton, 1961); Alfred H. Conrad and John R. Meyer, *The Economics of Slavery and Other Studies in Econometric History* (Chicago, 1964); Eric H. Erikson, *Childhood and Society* (2d ed., rev., New York, 1963); Robert W. Fogel, *Railroads and American Growth* (Baltimore, 1964); David V. Glass and D. E. C. Eversley, eds., *Population in History: Essays in Historical Demography* (Chicago, 1965); Peter Laslett, *The World We Have Lost: England before the Industrial Age* (New York, 1965); George Rudé, *The Crowd in History: A Study of Popular Disturbances in France and England, 1730–1848* (New York, 1964); Lawrence Stone, *The Crisis of the Aristocracy, 1558–1641* (Oxford, 1965); Stephan Thernstrom, *Poverty and Progress: Social Mobility in a Nineteenth-Century City* (Cambridge, Mass., 1964); E. P. Thompson, *The Making of the English Working Class* (London, 1964); and Charles Tilly, *The Vendée* (Cambridge, Mass., 1964). For a work published earlier, which became very influential during this period, see Stanley Elkins, *Slavery: A Problem in American Institutional and Intellectual Life* (Chicago, 1959).

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Precision and expertise for what? What is the "new social history"? The use of statistics? The adoption of concepts and the modes of argument of the social sciences? Are either of these approaches compatible with the traditional chronological emphasis of the discipline and with its venerable rhetorical mode of presentation? Beginning in the eighteenth century, Leo Braudy has suggested, "both novelists and historians sought to form time, to discover plot, and to give compelling and convincing narrative shape to the facts of human life." This admiration for a chronological form of presentation and a literary prose style and close attention to the values and motives of individual actors have come to constitute the dominant historiographical tradition in the United States. Most treatments of the American past, according to C. Vann Woodward, are "narrative, largely nonanalytical" accounts that are judged by the profession "according to old fashioned canons and values: thoroughness of research, objectivity of view, and clarity of logic, together with the lucidity and grace of the writing."[3]

Few of the new social historians have categorically rejected these standards, but their own work represents a fundamental reorientation of many of the traditional concerns of the discipline. Those historians with mathematical or logical skills—the "cliometricians"—have defined counterfactual propositions or hypotheses that can be tested and have then subjected historical data to elaborate statistical analyses. The research of these social science historians has thrown new light on patterns of congressional voting, the social correlates of political identity, and many aspects of economic development.[4] Other scholars have eschewed quantitative methods and have instead embraced the analytic approaches of contemporary social theorists. This interdisciplinary emphasis has been equally enlightening. Historians of the family have greatly expanded the dimensions and importance of their subject by exploiting theories derived from anthropology, demography, psychology, rural sociology, and labor economics. Scholars of urban history have likewise benefited from sociological approaches to stratification, geographical perspectives on building patterns, and architectural conceptions of urban esthetics.[5] While receptive to these statistical and theoretical techniques, another group of social historians has pursued a different goal: the resuscitation of the critical democratic scholarship of the Progressive era. By focusing on the lives of the vast majority of the American people, these authors have viewed the discipline "from the bottom up," exploring—indeed, in

[3] Braudy, *Narrative Form in History and Fiction* (Princeton, 1970), 3; and Woodward, "History and the Third Culture," *Journal of Contemporary History*, 3 (1968): 24, as quoted in David S. Landes and Charles Tilly, eds., *History as Social Science* (Englewood Cliffs, N.J., 1971), 35. Also see Gilbert Shapiro, "Prospects for a Scientific Social History," *Journal of Social History*, 10 (1976): 196–204.

[4] For a survey of some of this literature, see Van Beck Hall, "A Fond Farewell to Henry Adams: Ideas on Relating Political History to Social Change during the Early National Period," in James Kirby Martin, ed., *The Human Dimensions of Nation Making: Essays on Colonial and Revolutionary America* (Madison, Wisc., 1976), 323–71. For continuing discussions of method by these historians, see the *Historical Methods Newsletter* (1968–) and *Social Science History* (1976–).

[5] See, in particular, the articles and reviews published in the *Journal of Interdisciplinary History* (1970–). For some examples of this approach, see Tamara K. Hareven, ed., *Family and Kin in Urban Communities, 1700–1930* (New York, 1977); Thomas Bender, *Toward an Urban Vision: Ideas and Institutions in Nineteenth-Century America* (Lexington, Ky., 1975); and David Ward, *Cities and Immigrants: A Geography of Change in Nineteenth-Century America* (New York, 1971).

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many cases, discovering—the historical behavior and consciousness of immigrant workers, blacks, Native Americans, and women.[6]

The multiplicity of aims and methods is clearly evident. The "new social history" in the United States does not resemble a coherent subdiscipline but rather a congeries of groups—cliometricians, interdisciplinary social theorists, and critically minded social democrats—with often complementary and sometimes contradictory approaches to historical scholarship. The diversity of substantive interests is less striking than the sharp lines of methodological cleavage. Is a historical statement "true" only if it meets the test of rigorous statistical proof? Can the determination of individual motivation be attempted without an explicitly stated and internally consistent psychological theory? Does the behavior of a social group that has left no written documents provide adequate evidence of its values and goals? These questions have profound interpretive significance, for they address the legitimacy of various modes of argument and the admissibility of different types of evidence. Many of the new social historians begin from contradictory epistemological premises, accept divergent standards of proof, and argue creatively only with those who share their assumptions—as the confused controversy over the character of black consciousness under slavery amply demonstrates. The alternative is not an agreed-upon epistemological prolegomenon (for that is a chimera) but rather an ongoing debate among American social historians that confronts these issues in a direct manner and a search for a philosophy of historical analysis that combines methodology, social theory, and political ideology.

This essay addresses these issues in four ways. First, an explication of two modes of European scholarship—the French *Annalistes* and the English Marxists—demonstrates the feasibility of coherent intellectual systems of historical inquiry. Second, this two-part discussion begins the task of defining the prime features of social history and sets the stage for the third section of the paper: an investigation of the epistemological assumptions of most historians born in the United States. The weaknesses of the American tradition of pragmatic analysis are stressed in some detail, but the emphasis falls finally upon the great strength of this approach—a phenomenological perspective that depicts the historical experience "as it was actually lived" by men and women in the past. Fourth, the argument concludes with a discussion of various modes of historical discourse and the presentation of an "action model," a rhetorical strategy and methodological synthesis designed to reconcile the divergent social scientific, interdisciplinary, and ideological tendencies of the "new social history" written in the United States during the past two decades.

"SOCIAL HISTORY IS NOT A PART OF HISTORY," the British scholar Harold J. Perkin has argued, but "all history from the social point of view." Eric J. Hobsbawm

[6] Lemisch, "The American Revolution Seen from the Bottom Up," 3–45; and Alfred F. Young, ed., *Dissent: Explorations in the History of American Radicalism* (DeKalb, Ill., 1968), and *The American Revolution: Explorations in the History of American Radicalism* (DeKalb, Ill., 1976). Also see the increasingly influential *Radical History Review* (1973–).

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has concurred; because the "societal aspects of man's being cannot be separated from other aspects of his being," social history consists of the holistic analysis of "specific units of people living together and definable in sociological terms." These definitions are instructive but limited, at least when viewed from across the English Channel. They focus, according to Richard T. Vann, on human aggregations, "churches, sects, social classes, village communities, and . . . the family" rather than on the geographic or environmental regions that form the "central subjects" of the *Annales* school of French historians.[7] Consider, for example, Fernand Braudel's minute attention to the seasonal rhythms of the Mediterranean world of the sixteenth century. All of the peace treaties dated from the winter months; conversely, "with summer's coming, war sprang to life in all its forms: land warfare, galley warfare, pirate attacks at sea, and brigand raids in the countryside." These regularities were clearly understood by contemporaries and often figured prominently in people's behavior. "The banks of Naples," Braudel pointed out, "regularly in winter months invested their clients' money in government bonds whereas in summer they used it to buy up the many agricultural products of the kingdom, a profitable speculation."[8] Such patterns of existence were pervasive; they impinged, however unequally, upon all of the social groups in the region.

This stress on the prime structural features of social, technological, and cultural life within a specific geographic environment defines the approach of Braudel and of other *Annalistes*. Indeed, their geographic and structuralist approach represents a distinct intellectual system, a conception of history that is at once a method, a subject matter, and an interpretation. "What is the philosophy of the *Annales* school?" Hugh R. Trevor-Roper asked in 1972. He isolated three interrelated elements. First, there was the use of quantification, "to reduce the area of incomprehension by rigorous statistical analyses." Second, the *Annalistes* sought "to grasp the totality" and "the vital cohesion of any historical period" by delineating all of its structures. Finally, these French historians inclined toward a "social determinism," a belief that "history is at least partially determined by forces which are external to men." As Braudel himself noted, the structures of history ("chains of small facts indefinitely repeated") live on "for so long that they become stable elements for an indefinite number of generations; they incumber history, they impede and thus control its flow."[9]

This intricate web of method, theory, and philosophy gives intellectual coherence to French social history, and a distinctive interpretive position emerges as a result of the definitions given to three terms: quantification, totality, and struc-

[7] Perkin, "Social History," in Fritz Stern, ed., *The Varieties of History: From Voltaire to the Present* (2d ed., New York, 1973), 433; Hobsbawm, "From Social History to the History of Society," in T. C. Smout and M. L. Flinn, eds., *Essays in Social History* (London, 1974), 5; and Vann, "The Rhetoric of Social History," *Journal of Social History*, 10 (1976): 222.

[8] Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, trans. Siân Reynolds, 2 vols. (New York, 1975), 1: 257, 266.

[9] Trevor-Roper, "Fernand Braudel, the *Annales*, and the *Mediterranean*," *Journal of Modern History*, 44 (1972): 466–71; and Braudel, *Capitalism and Material Life, 1400–1800*, trans. Miriam Kochan (New York, 1975), 442, and "History and the Social Sciences," in Peter Burke, ed., *Economy and Society in Early Modern Europe: Essays from* Annales (New York, 1972), 13, 17–18.

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ture. The use of statistical methods by the *Annales* historians, Georg G. Iggers has suggested, proceeds from their attempt to comprehend the past in causal terms. This positivist approach reflects the influence of Emile Durkheim on Marc Bloch, one of the founders of the *Annales* school. Following the great social theorist, Bloch assumed that "society manifested itself in concrete forms which could be observed from the outside very much like the phenomena of nature."[10] Because of Bloch's profoundly historical orientation, however, this social scientific premise did not result in a search for statistical regularities or general historical laws. The prime concern of the early *Annalistes* was the unique character of past civilizations, and during the 1930s and 1940s most of the historians influenced by Bloch—and by Lucien Febvre—concentrated on a close examination of literary documents. Their goal was the discovery of the intellectual and psychological climate of an age, the *mentalité* of individuals and social groups. Only in the 1950s did quantification and heavy reliance on economic documents begin to take precedence, and by then the emphasis on traditional types of historical questions was well established. As a result, statistical expertise did not become an end in itself, the technical fetish that has distorted the focus of the work of some American cliometricians. Rather, the *Annalistes* subordinated the quantitative mode to their historical perspective, examining various aspects of the past in their own terms: the trade of Seville in the sixteenth century, the cycles of life, death, and subsistence in Beauvais between 1600 and 1730, the movement of agricultural prices on the eve of the French Revolution.[11]

If the later *Annalistes* successfully integrated chronology and quantification, they were unable to solve the problem of focus. The research of the early French social historians—of Bloch, Febvre, and their contemporaries—was innovative in its choice of subjects and wide in analytic scope. As J. H. Hexter has suggested, much of their scholarship fell under the rubric of *histoire problèm.* It demonstrated the "integral relationships among all facets of human existence" but did so within the comprehensible boundaries of a traditional monograph; the broad approach was balanced by a precise focus. Then, with the publication of Braudel's monumental treatise on *The Mediterranean and the Mediterranean World in the Age of Philip II* in 1949, both the scale of the analysis and its philosophical underpinnings changed dramatically. Braudel's appetite for the past (in "its largest lineaments and its most intimate details") was Gargantuan. And he translated this personal vision into a method, "a new kind of history, total history."[12] All aspects of life—from climate to topography to architecture, from popular culture to capitalist values to high art—were sketched in to create a comprehensive multidimensional Cubist portrait of the society. The superfluity of detail and the multiplicity of perspectives inhibit easy interpretation. The inherent diffuseness of *histoire totale* is apparent even to its advocates. "The more

[10] Iggers, *New Directions in European Historiography* (Middletown, Conn., 1975), 50, chap. 2 *passim.*

[11] Pierre Chaunu and Huguette Chaunu, *Seville et l'Atlantique: Partie Statistique*, 8 vols. (Paris, 1955–57), and *Seville et l'Atlantique: Partie Interpretive*, 4 vols. (Paris, 1959–60); Pierre Goubert, *Beauvais et le Beauvaisis de 1600 à 1730: Contribution à l'histoire sociale de la France du XVII^e siècle* (Paris, 1960); and Ernest Labrousse, *La crise de l'economie française à la veille de la Révolution* (Paris, 1944).

[12] Hexter, "Fernand Braudel and the *Monde Braudellien* . . . ," *Journal of Modern History*, 44 (1972): 523, 537–38.

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aspects you study," Theodore Zeldin has observed with respect to his own massive history of modern France, "the more specialization there is, and the more life becomes a mass of 'factors' and 'influences' with no unifying theme." Method shades imperceptively into philosophy. The past consists of a chaotic mass of facts and details, Zeldin concluded, "a vast number of permutations, the divisions between men cutting across each other."[13]

Braudel did not embrace this stark empiricist position but instead sought pattern and meaning in the historical record itself. The positivist heritage of the early *Annalistes* was undoubtedly an important reason for this choice but so, too, was the growing importance of structuralist modes of analysis in French intellectual life. Ferdinand de Saussure, the influential early twentieth-century philologist, demonstrated the crucial difference between *langue*, the basic system of grammatical rules and conventions, and *parole*, the ordinary act of speech that unconsciously (but unfailingly) embodies these norms. Subsequently, the eminent anthropologist Claude Lévi-Strauss proposed a similar analytic distinction between "social relations," the data of the empirical reality of human life, and "social structure," the "models which are built up after it." For de Saussure and Lévi-Strauss the task of the scholar was to decode reality, to penetrate beneath the complex surface of human existence to its basic structures.[14] Braudel proceeded to apply this analytic framework to the history of the vast Mediterranean world. Amid the diversity and sheer human chaos of his massive subject the new dean of the *Annales* school isolated three distinct structures or "conceptions of time." The first and most basic was geographic or environmental time, "those local, permanent, unchanging and much repeated features which are the 'constants' of Mediterranean history." These enduring patterns of settlement, subsistence, and social relations were overlaid during the early modern period by a "long duration" of specific institutions. Some of these formal organizations and patterns (tax systems, technology, the incidence of disease and famine) "barely moved in any significant way, from the fifteenth to the eighteenth century"; but there were also a number of medium-term trends or cycles in economic and social life, "conjunctures" such as the price inflation of the sixteenth century and fluctuations in the rate of demographic change. Finally, Braudel turned to the conception of time that gave prime attention to "the short term, the individual, and the event," the political acts and diplomatic maneuvers that were the subjects of traditional history. The last third of *The Mediterranean and the Mediterranean World in the Age of Philip II* renders in great detail the political struggles, dynastic wars, and personalities that dominated the historical surface of the era. By delineating these three different structures of human life and activity, the great French historian hoped to make manifest their relationships: "to

[13] Zeldin, "Social History and Total History," *Journal of Social History*, 10 (1976): 243–44, 244.

[14] Lévi-Strauss, *Structural Anthropology*, trans. Claire Jacobson and Brook Grundfest Schoepf, 1 (New York, 1963): 279, chap. 15 *passim*. Also see de Saussure, *Course in General Linguistics*, ed. Charles Bally and Albert Sechehaye, trans. Wade Baskin (New York, 1959); and Howard Gardner, *The Quest for Mind: Piaget, Lévi-Strauss, and the Structuralist Movement* (New York, 1972).

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acquaint the reader with their coexistence, the conflicts, and contradictions, and the richness of experience they hold."[15]

This impressive intellectual scheme contains two significant flaws. Although the concept of time-related structures assists historians in ordering the disparate data of social life, the simple three-fold classification is reductive. The categories cannot encompass the diversity and complexity of reality. As Braudel himself has now acknowledged, there are "many measures of time, . . . each of them attached to a particular history." If this revision obviates one major criticism and significantly increases the analytic precision of the structuralist approach, it also accentuates the second major methodological weakness. *The Mediterranean and the Mediterranean World in the Age of Philip II* does not demonstrate how the historian might link the various levels of reality, to tie "the durable phenemona of history with those that involve rapid change." The long chapter on the battle of Lepanto, for instance, takes the form of a traditional political and military narrative; Braudel has not directly specified how its causes or outcome were affected by the social and economic patterns minutely examined in the preceding four hundred pages. The three dimensions of existence—geographic, social, and political—are not interrelated but simply stand one beside the other, "each . . . itself an essay in general explanation."[16]

This failure to specify causal relationships is neither accidental nor peculiar to Braudel's scholarship; rather, it proceeds directly from the method. To interpret the world in structuralist terms is to contest the philosophical primacy of nineteenth-century notions of unilineal causation. The understanding of an event depends less on a comprehension of its antecedents than on an understanding of its position or function within an existing system. Structuralists adopt a "holistic" perspective, stressing the internal relationships among elements of a self-contained institution or world view. Thus, the controversial French intellectual Michel Foucault has rejected a "history" of ideas—in Arthur O. Lovejoy's sense of chronological transmission and essential continuity—in favor of an "archeology" of knowledge. For Foucault the past consists of separate strata or "epistemes," each with its own internally consistent paradigmatic vision. Certain concepts may be inherited from the past, but their significance is radically transformed by the new context of meaning; the study of antecedents gives way to an analysis of relationships. Although Foucault's dismissal of the intrinsic value of the discipline of history is extreme, it reveals the tension between a structuralist form of presentation and a chronological depiction of a causal sequence.[17] A graphic illustration of the implications of the structuralist approach appears in the organization of Robert Mandrou's stimulating *Introduction to Modern France,*

[15] Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, 2: 892–900, 1238–39, 1244, and *Capitalism and Material Life, 1400–1800*, x.

[16] Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, 2: 1238; and Hexter, "Fernand Braudel and the *Monde Braudellien*," 534.

[17] Foucault, *The Archeology of Knowledge*, trans. A. M. Sheridan Smith (New York, 1972), and *The Birth of the Clinic: An Archeology of Medical Perception*, trans. A. M. Sheridan Smith (New York, 1973). For an incisive explication and critique, see Hayden V. White, "Foucault Decoded: Notes from Underground," *History and Theory*, 12 (1973): 23–54.

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1500–1640. Although Mandrou conceded that "the precariousness of material life or chronic malnutrition cannot be understood without reference to techniques of agricultural production, or to the system by which land revenues were distributed" and admitted "the great degree of artificiality" in relegating these topics to different parts of his book, he ultimately chooses to present his material in this causally unrelated manner. Like many other *Annalistes* Mandrou is primarily interested in the pattern of existence.[18] Seen in this context, Braudel's failure to link "event, conjuncture, and structure" is less an interpretive weakness than a distinctive feature of a national mode of intellectual discourse.

The structuralist method accounts as well for the sense of "determinism" that Trevor-Roper has detected in *Annales* history. Long-term patterns of stable relationships under the *Ancien Régime* are described by the French social historians both as empirical regularities and as reified constraints on human choice and action. "Man's whole life is restricted by an upper limit," Braudel declared at one point, a boundary which is "always difficult to reach and still more difficult to cross." This profoundly tragic view of man as "trapped in his former achievements for generations on end" has deeply influenced the outlook of other scholars. Pierre Goubert has written of the eternal cycle of famine and death in the pre-industrial world, while Emmanuel Le Roy Ladurie has emphasized both the limiting effects of climate on agricultural production and the prevalence of backward *mentalités*, the "invisible spiritual frontiers which constrained economic growth." The decisive role of consciousness has been fully developed by Mandrou, who has presented the history of France between 1500 and 1640 in terms of the "basic forces for solidarity," the constraining intellectual and institutional environment that "closely circumscribed everyday human activities within a web of tradition." Whether geographic, biological, or cultural, the structures of the *Annalistes* imprison the individual "within a destiny in which he himself has little hand." History reveals a record of forced accommodations, for every choice is made within the terms inherited from the past. As Braudel has concluded, "All efforts against the prevailing tide of history . . . are doomed to failure."[19]

The various French aspects of *histoire totale*—its tragic sense of existence, its emphasis on stability and continuity, and its diffuse structuralist treatment of causality—restrict its appeal and its range. The scholarly weight of the *Annalistes* remains anchored in the realities of the pre-industrial world, in the lives of human beings who "move within the strict confines of a physical space, obey impulses, and submit to barely visible forces of tradition." By contrast, many Anglo-American social historians are concerned with processes of change and conflict, with the political and economic revolutions of the post-1750 era. As

[18] Mandrou, *Introduction to Modern France, 1500–1640: An Essay in Historical Psychology*, trans. R. E. Hallmark (Paris, 1961, 1974; New York, 1976), 72, *passim*.

[19] Braudel, *Capitalism and Material Life, 1400–1800*, 25, ix; Le Roy Ladurie, *The Peasants of Languedoc*, trans. John Day (Urbana, Ill., 1974), 269; Mandrou, *Introduction to Modern France, 1500–1640*, 78; and Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, 2: 1244. Also see Goubert, *Beauvais et le Beauvaisis de 1600 à 1730*; and Le Roy Ladurie, *Times of Feast, Times of Famine: A History of Climate since the Year 1000*, trans. Barbara Bray (Garden City, N.Y., 1971).

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Richard Mowery Andrews has astutely pointed out, the dominant philosophical assumptions of English Marxists and American liberals alike "have been voluntarist—postulated on the notion that human will, moral and pragmatic, determines social and political realities."[20] Yet to stress regularity and structure is not necessarily to endorse a predictive determinism or to negate the existence of variation, contradiction, and conflict. Any "total" view of society will demonstrate the multiplicity of relationships and the diversity of cultural patterns. Choice takes place in the interstices of this social matrix, in a fashion suggested by the English structural anthropologist Frederick George Bailey. He has regarded the "structure" of the social environment "not so much as made up of relationships which are obligatory upon the various actors, but rather as imposing some kind of limit on possible action, while leaving within these limits an area of discretion and choice, which the actors can manipulate in order to achieve their ends."[21] Whatever the philosophic force of this apologia, the pessimistic bias of the *Annales* school cannot be ignored. Men and women act out their lives within the confines of a rigid system, and these structures of existence are often the real subjects of analysis—statistically plotted and totally explicated in a distinctly French rationalist tradition.

CONTEMPORARY MARXIST SCHOLARSHIP, like that of the French structuralists, begins from a positivist premise. "The subject matter of history," declared the French Marxist Pierre Vilar, "is structured and accessible to thought, is scientifically penetrable like any other sort of reality." As for the *Annalistes*, this epistemological position represents merely a single facet of a complex intellectual system. To adopt a Marxist perspective toward history is to acknowledge the essential validity of Marx's critique of the development of capitalism. This entails four propositions. The first analytic principle states that the productive system is of crucial importance in the life of a society, and the second asserts that the social relations of production are manifest in class divisions. The third Marxist axiom posits change and contradiction as fundamental features of social reality and explains historical change in terms of the dialectical process first specified by Hegel. The final proposition insists that capitalist social relations alienate men and women from both their labor and their inherent selves. This moral supposition justifies the methodology and defines its task. The Marxist scholar

[20] Andrews, "The Mediterranean," *New York Times*, May 18, 1975, sec. 7, p. 1. For a supplementary analysis, see Iggers, *New Directions in European Historiography*, 65–79. The work of the *Annales* school has been most influential among scholars of the pre-1750 period of American history. These historians have emphasized historical change to a greater extent than their French counterparts but have placed these alterations *within* the established structures of the world of pre-industrial agriculture. See Kenneth A. Lockridge, "Land, Population, and the Evolution of New England Society, 1630–1790," *Past & Present*, no. 39 (1968): 62–80, and *A New England Town: The First Hundred Years—Dedham, Massachusetts, 1636–1736* (New York, 1970); and Philip J. Greven, Jr., *Four Generations: Population, Land, and Family in Colonial Andover, Massachusetts* (Ithaca, N.Y., 1970).

[21] Bailey, *Tribe, Caste, and Nation: A Study of Political Activity and Political Change in Highland Orissa* (Manchester, 1960), 11–12. As H. Stuart Hughes has suggested, "Some such conviction of the inevitable limitations on human freedom—whether by physical circumstances or through emotional conditioning—has become the unstated major premise of contemporary social science"; Hughes, *Consciousness and Society: The Reorientation of European Social Thought, 1890–1930* (New York, 1958), 4.

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must achieve a critical, theoretical understanding of the historical development of productive systems, class divisions, and the process of change so that the present social order can be comprehended and then transformed.[22]

Many American social historians categorically reject a Marxist theoretical approach while finding much to praise in the writings of such leading Marxists as Eugene D. Genovese, Christopher Hill, Eric J. Hobsbawm, and Edward P. Thompson. This paradox stems in part from the mistaken identification of Marxism with doctrinaire scholarship and a "vulgar materialist" approach to historical interpretation. Yet these Marxist authors differ significantly with respect to analytic emphasis and causal explanation. And, as their American readers have clearly recognized, the arguments of these scholars are not reductive in the least; their interpretations are based on a mass of empirical evidence and demonstrate a mastery of the extant primary documents.[23] Indeed, with respect to the charge of economic determinism, the antagonists have switched sides. Contemporary Marxists have accused the "new economic historians"—liberal American academics whose quantitative methods and causal reasoning have often assumed the primacy of economic motivation and market forces—of reductive scholarship, a criticism echoed by many non-Marxist writers.[24] Given this blurring of old ideological lines, it is necessary to explore the dimensions of recent Marxist historiography and to comprehend its approach to the historical process.

In an interesting explication of "Base and Superstructure" in Marxist theory, Raymond Williams has re-examined the traditional categories of Marxist analysis. He began by pointing out the limiting effects of "the language of determination and even more of determinism" that Marx inherited from theological arguments regarding free will. This heritage, Williams has suggested, accounts for the notions of "prefiguration, prediction, or control" in some of Marx's writings. Williams then advanced a concept of causal explanation derived from social experience rather than from religious or philosophical ratiocination, of forces or institutions "setting limits, exerting pressures" on human action.[25] He has also reassessed other terms. To speak of a cultural or ideational superstructure that "reflects" an economic or material base is to succumb to the anachronistic philosophic concepts and linguistic usages of the nineteenth century. "What is wrong with all questions about the relation of 'subjectivity' to the world," the Polish-born philosopher Leslek Kolakowski has argued in a related discussion of the etiology of knowledge, "is that we are not able to express them or answer

[22] Vilar, "Marxist History, a History in the Making: Towards a Dialogue with Althusser," *New Left Review*, 80 (1973): 66, as quoted in Iggers, *New Dimensions in European Historiography*, 144. For a concise statement of Marxist analytic principles, see E. J. Hobsbawm, "Karl Marx's Contribution to Historiography," in Robin Blackburn, ed., *Ideology in Social Science* (Bungay, 1972), 265–83.

[23] See, for example, Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1973); Hill, *The World Turned Upside Down: Radical Ideas during the English Revolution* (New York, 1972); Hobsbawm, *Primitive Rebels: Studies in Archaic Forms of Social Movement in the 19th and 20th Centuries* (New York, 1959); and Thompson, *The Making of the English Working Class*.

[24] Iggers, *New Dimensions in European Historiography*, 139, 165; and Michelle Perrot, "The Strengths and Weaknesses of French Social History," *Journal of Social History*, 10 (1976): 174.

[25] Williams, "Base and Superstructure in Marxist Cultural Theory," *New Left Review*, 82 (1973): 3–16.

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them except with the help of spatial symbols, while we know that what matters are not topological relations." "Expressions like 'in the conscousness' . . .," Kolakowski concluded, "hinge necessarily on this spatial language, and they cannot get at the literal form."[26]

Recognizing these conceptual difficulties, Williams has proposed two solutions. The first alternative (and, in a philosophic sense, the more radical) renounces nineteenth-century notions of unilineal causality because of their inherent tendency to depict ideas as epiphenomena of social reality and substitutes a "theory of social totality." This holistic approach replaces the "formula of base and superstructure" with the "more active idea of a field of mutually if also unevenly determining forces." Thus, Williams has argued that

> we have to revalue "determination" toward the setting of limits and the exertion of pressure, and away from a predicted, prefigured and controlled content. We have to revalue "superstructure" toward a related range of cultural practices, and away from a reflected, reproduced or specifically dependent content. And, crucially, we have to revalue "the base" away from the notion of a fixed economic or technological abstraction, and toward the specific activities of men in real social and economic relationships, containing fundamental contradictions and variations and therefore always in a state of dynamic process.[27]

This theoretical position has been elaborated, in various forms, by other Marxists. Antonio Gramsci has proposed "the notion of 'historic bloc,' in which the material forces are the content and ideologies the form—merely an analytical distinction since material forces would be historically inconceivable without form. . . ." As Martin Jay has explained with great precision, similar "analogical" solutions to the problem of the sociology of knowledge were devised by non-Marxists such as Karl Mannheim and the neo-Hegelian Marxists of the early Frankfurt School. This "totalistic view of cognition" implicitly underlies Williams's second, and more specific, alternative. The historian can retain the concepts of base and superstructure but focus on the point at which they intersect in people's lives. Through the process of "mediation," historical actors give cultural significance to their material existence; their condition constantly forces them to resolve the inherent contradictions between present circumstances and received wisdom.[28]

Ironically enough in view of its widespread identification with economic determinism, Marxist historiography invariably addresses this complex relationship between life and thought—the cognitive aspects of social action. Marxist scholars have stressed the "role of consciousness as an active factor in social

[26] Kolakowski, *Husserl and the Search for Certitude* (New Haven, 1975), 80–81. Following Henri Bergson, Kolakowski has argued that these difficulties constitute "a lasting feature of language itself"; *ibid.*, 81. For a similar analysis of Durkheim's conceptual and linguistic problems, see Hughes, *Consciousness and Society*, 283.

[27] Williams, "Base and Superstructure in Marxist Cultural Theory," 5–6. Williams has explored other implications of his position in the Introduction to Lucien Goldmann, *Racine*, trans. Alastair Hamilton (Cambridge, 1972), iii–xxii.

[28] Gramsci, *Il materialismo storico e la filosofia di Benedetto Croce: Opere*, 2 (Turin, 1949): 49, as quoted in Eugene D. Genovese, "Marxian Interpretations of the Slave South," in Bernstein, *Towards a New Past*, 98; Williams, "Base and Superstructure in Marxist Cultural Theory," 7, 10–11; and Jay, "The Frankfurt School's Critique of Karl Mannheim and the Sociology of Knowledge," *Telos*, 20 (1974): 72–89.

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change" and have been in the forefront of the broader movement that Lawrence Stone described as "a new, more sophisticated, and subtle attempt to link intellectual history with a social matrix." The distinct Marxist contribution to this literature consists in the exploration of the values and political consciousness of various oppressed social groups: black slaves in the United States, primitive rebels in marginal economic regions of modern Europe, merchant seamen during the American War for Independence, millennial revolutionaries in seventeenth-century England, and the pre-industrial European crowd.[29]

This new methodological and interpretive perspective has transformed our understanding of the process of social conflict during the transition to capitalism in early modern Europe and America. To take one prominent example, E. P. Thompson has demonstrated the existence of a definite set of social values among many participants in the food riots that swept across eighteenth-century England, a conception of a "moral economy" that informed their actions. "Being hungry," Thompson has asked, "what do people do? How is their behavior modified by custom, culture, and reason?" His careful examination of "the complexities of motive, behavior, and function" prompted him to reject a narrow materialist interpretation of these uprisings as "a direct, spasmodic, irrational response to hunger." Instead, he has argued that these actions of the crowd were purposeful attempts to preserve the traditional notion of a "just price" in an age of agricultural transition; bakers and grain merchants were often forced by public pressure to sell their goods at the "usual" price, rather than at a level determined by the market forces of supply and demand. If a Marxist perspective informs Thompson's account and enables him to see that these riots represented a conflict between long-standing social values and the imperatives of a new system of political economy, the interpretation is very much his own. The struggle between the landless working classes and the capitalists who controlled the supply of food, he suggested, was fought out partially in cultural terms, as a conflict between opposed conceptions of the ideal social order. "To say that it was 'cultural' is not to say that it was immaterial, too fragile for analysis, insubstantial," Thompson argued, conducting an implicit dialogue with more materialist-oriented Marxist scholars like Louis Althusser on the one hand and with idealist symbolic anthropologists on the other. Rather, to say that the conflict was "cultural" is to analyze the different modes of production in terms of the consciousness of the historical actors themselves—as "images of power and authority, the popular mentalities" of hegemony and resistance.[30]

[29] Iggers, *New Directions in European Historiography*, 152; and Stone, as recorded in "Dialogues: New Trends in History," *Daedalus*, 98 (1969): 957. Also see Rudé, *The Crowd in History*; Jesse Lemisch, "Jack Tar in the Streets: Merchant Seamen in the Politics of Revolutionary America," *William and Mary Quarterly*, 3d ser., 25 (1968): 371–407; and the works cited in note 23, above.

[30] Thompson, "The Moral Economy of the Crowd in Eighteenth-Century England," *Past & Present*, no. 50 (1971): 77–79, 135–36, and "Patrician Society, Plebian Culture," *Journal of Social History*, 7 (1974): 387. To ascertain the distinctiveness of Thompson's position, it is necessary only to compare the formulation of the French Marxist Louis Althusser—"in its religious, ethical, legal, and political forms, etc., ideology is an objective reality; the ideological struggle is an organic part of the class struggle"—with that of Antonio Gramsci—"revolutionary thought does not see time as a factor of progress. . . . To pass through one stage and advance to another, it is enough that the first stage be realized in thought." Yet Gramsci's position is less Idealist than the semiotic definition of culture advanced by the American symbolic anthropologist Clifford Geertz: "man is an

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Manifest in Thompson's superb treatment of the behavior and values of the eighteenth-century English crowd is a distinct and coherent intellectual position. His theoretical understanding constantly informs his analysis and prompts him to examine certain types of social relationships: property, class, and power. Yet, like other contemporary Marxist scholars, Thompson has accepted the historical integrity of his data; they need not conform to a simple schema. "The general theory of historical materialism," Eric Hobsbawm has indicated, "requires only that there should be a succession of modes of production, though not necessarily any particular modes, and perhaps not in any particular predetermined order." Such formulations admit the complexity of a given historical situation while advancing two interpretive principles. The first proposition states that "the analytic base of any historical inquiry . . . must be the process of social production."[31] This position should not be confused with narrow materialism. "There is no excuse for identifying the economic origins of a social class with the developing nature of that class," Eugene Genovese has explained, for "every social class . . . [is] the product of a configuration of economic interests, a semi-autonomous culture, and a particular world outlook." All aspects of life form parts of a Marxist approach, but the disparate patterns of existence are subordinated to the basic configuration, "the way the given society maintains and transforms itself—its manner of social reproduction."[32]

An investigation of the modes of production necessarily involves a second major analytic concern: the isolation of the dominant system of relations within the society. There are many structures of belief and behavior, as the *Annalistes* correctly insist, but some are more important than others. "Historians," Hobsbawm declared, "will be tempted—and in my view rightly—to pick on one particular relation or relational complex . . . , and to group the rest of the treatment around it—for example Bloch's 'relations of interdependence' in his *Feudal Society*." As Hobsbawm's choice of illustration suggests, the quest for a central relational complex does not constitute a distinctively Marxist perspective but becomes so only when combined with an emphasis on the process of social production, the hierarchical arrangement of the structures of life and the distribution of authority within the society, and "the existence of internal contradictions within systems, of which class conflict is merely a special case."[33]

This concern with power and change clearly differentiates the Marxist analytic framework from that of the later *Annalistes*, with its static assumptions,

animal suspended in webs of significance he himself has spun." See Althusser, *For Marx*, trans. Ben Brewster (New York, 1969), 11–12; Gramsci, as quoted in Eugene D. Genovese, *In Red and Black: Marxian Explorations in Southern and Afro-American History* (New York, 1971), 393; and Geertz, *The Interpretation of Cultures* (New York, 1974), 5. Also see Iggers, *New Dimensions in European Historiography*, 165–71.

[31] Hobsbawm, "From Social History to the History of Society," 10, and *Pre-Capitalist Economic Foundations* (New York, 1965), 19–20, as quoted in Iggers, *New Dimensions in European Historiography*, 205 n. 35.

[32] Genovese, "Marxian Interpretations of the Slave South," 97, 98; and Lisa Vogel, "Rummaging through the Primitive Past: A Note on Family, Industrialization, and Capitalism," *Newberry Papers in Family and Community History*, no. 76–2 (1976): 1–2.

[33] Hobsbawm, "From Social History to the History of Society," 10–11, and "Karl Marx's Contribution to Historiography," 273–74. For a similar formulation, see Perry Anderson, *The Lineages of the Absolutist State* (London, 1974), 423.

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causal diffuseness, and apolitical stance. Marxists not only specify economic and cultural structures but also relate them to one another and demonstrate patterns of confrontation among them. Most Marxists also restrict the scope of their research to an issue with a defined focus (the device of *histoire problème*), in part because they wish to emphasize the personal dimensions of historical experience. Many Marxist scholars approach their material with a moral perspective derived from the Romantic critique of capitalism and from the early, "humanistic" writings of Marx. This social philosophy portrays men and women as alienated from their inherent natures by forces that they cannot always control and depicts them as struggling valiantly, if often unsuccessfully, against the impact of historical developments that threaten their autonomy and their traditional way of life. However sober and realistic the analysis, the ideological message is optimistic: the world can be transformed by purposeful action. This precept applies to the present as well as the past, and imposes a duty upon the historian. Marxist scholarship seeks to transcend the often sterile limits of scientific "objectivity," to infuse historical writing itself with values, and to discuss the morality as well as the actuality of past events. As Thompson has argued, the historian must first determine the "values *actually held* by those who lived" through an epoch of the past and then must make "some judgment of value upon the whole process."[34] Manifest in Thompson's work and in that of many other Marxist scholars is a critical methodology and a moral purpose that American social historians might well emulate.

THE RELUCTANCE OF HISTORIANS born and trained in the United States to accept the analytic systems formulated by Marxists, *Annalistes*, and other European social theorists proceeds in part from the very different historical experiences of the various groups of intellectuals. "Braudel's post-voluntarist and post-imperial vision," Richard Andrews has pointed out, reflects the influence of "a culture that has finally assimilated the failure of its imperial ambitions and its positivistic optimisms." Whatever the nagging political doubts and philosophical uncertainties characteristic of American academe, this profound sense of the restricted limits of human endeavor is largely absent; as C. Vann Woodward has indicated, only southern-born Americans have personally participated in the common European experience of decades of intractable social conflicts and of crushing military defeat. If the relative national success of the United States inhibits its intellectuals from adopting the tragic vision of the *Annalistes*, then the absence of a viable working-class socialist movement has discouraged the development of a strong Marxist historiographic tradition. The predominance of bourgeois parties and values in twentieth-century American politics, Eugene Genovese has

[34] Thompson, *The Making of the English Working Class*, 444. John Higham has likewise urged historians to take a critical stance, "to assume an active responsibility both to a phase of the past and to a contemporary public, and to engage one with the other"; Higham, "Beyond Consensus: The Historian as Moral Critic," *AHR*, 67 (1961–62): 609–25. Also see Perkin, "Social History," 441; and E. P. Thompson's "Postscript: 1976" to his *William Morris: Romantic to Revolutionary* (1955; New York, 1977), esp. 790–807.

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suggested, encouraged politically minded academics to neglect "class forces and the process of capitalist development in favor of . . . the glorification of the Jefferson-Jackson-Roosevelt liberal tradition. . . ."[35]

These political realities found expression in intense ideological debates, especially during the period of the Cold War. The military confrontation between the socialist states and the capitalist nations produced an analogous conflict within the American historical profession. The validity of Marxism as an intellectual system was the crucial point at issue in this academic debate. "The way social change in fact took place is something we may find out by *using* our framework," J. H. Hexter proclaimed some two decades ago in a candid polemical attack on Marxist historiography; "it is not and *ought not be* something to be found *in* the framework."[36] Whatever its political motivation, Hexter's argument effectively rebutted reductionist scholarship in general and a "vulgar materialist" economic approach in particular; but contemporary Marxism is largely immune from such intellectual shortcomings. Of greater importance here is the legitimacy of Hexter's implied "empiricist" alternative to the Marxist framework. Serious epistemological questions immediately arise. Most American-born historians have silently imbibed a pragmatic philosophical perspective from their culture. They accept the primacy of sense perception and attempt to establish the "facts" in a "spirit of neutral, passive detachment." Their approach assumes a correspondence theory of truth; the relations among the empirical data are thought to be as real and as immediately apparent to the historian as the facts themselves. These are dubious propositions. As the philosopher of science Carl Hempel has pointed out, broad agreement exists among mid-twentieth-century philosophers that even "scientific hypotheses and theories are not derived from observed facts, but invented to account for them."[37] Thus, there is no ontological certainty that the external world corresponds to the models created by the scientist—or by the historian. Hexter's empiricist framework is, therefore, just as much of an intellectual invention as that proposed by a Marxist or, for that matter, the structuralist frames of reference preferred by de Saussure, Lévi-Strauss, and Braudel or the paradigmatic formulas of Michel Foucault and Thomas Kuhn. Any model that is "used" has certain assumptions about the nature of reality embodied "in" it.

Yet important differences do exist between the pragmatic empiricist position endorsed by Hexter and the epistemological assumptions underlying the European intellectual traditions of Marxism and structuralism. The latter are rationalist philosophies; they elevate the power of human reason and exalt its critical

[35] Andrews, "The Mediterranean," 44–45; Woodward, "The Irony of Southern History," in his *The Burden of Southern History* (Baton Rouge, 1960), 167–91; and Genovese, "Marxian Interpretations of the Slave South," 91.

[36] Hexter, "A New Framework for Social History," in his *Reappraisals in History: New Views on History and Society in Early Modern Europe* (London, 1961), 16.

[37] Cushing Strout, *The Pragmatic Revolt in American History: Carl Becker and Charles Beard* (New Haven, 1958), 8; and Hempel, *The Philosophy of the Natural Sciences* (Englewood Cliffs, N.J., 1966), 15, as quoted in Iggers, *New Dimensions in European Historiography*, 6. Also see Hughes, *Consciousness and Society*, 183–87; Harriet Gilliam, "The Dialectics of Realism and Idealism in Modern Historiographic Theory," *History and Theory*, 15 (1976): 231–56; and Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1970), 111–35, 167–70.

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function, while contesting the primacy of mere facts. Hence, Lévi-Strauss has attacked the "empirical and naturalistic" procedures of the eminent English anthropologist A. R. Radcliffe-Brown and has disputed his ability to understand the "social structure" of a community from an examination of its pattern of social relationships. "The term 'social structure,'" Lévi-Strauss asserted, "has nothing to do with empirical reality but with models which are built up after it." Similar rationalist postulates form the philosophical foundations of Marxism. As Stuart Hall has pointed out, Marx rejected a correspondence theory of truth; for, "if things were synonymous with their surface appearances, there would be no need for science."[38] Thus, when American-born historians raise the "facts" to a position of ultimate authority, they assume a distinct epistemological postion—an empiricist approach based on three interrelated propositions: that human reason has limited power to understand the world; that models or frameworks can comprehend only the immediate data to which they apply; and that there are no fundamental patterns or structures of human life. These premises lead directly to the conclusion that each historical case has to be treated on its own, as a unique constellation of specific conditions or events. The most general result that can be obtained is an "hypothesis," but this must be "tested" with respect to each new case. The "facts" remain supreme.

This inbred pragmatic American approach to historical reality is gradually being eroded by the subtle pressures exerted by various theoretical systems. The "functionalist" social theory of Talcott Parsons, for example, has deeply influenced the thinking of many social historians in the United States. Parsons' scholarship embodies an anti-empiricist bias, for it assumes the existence of systematic relations in the world of social reality. The social systems specified by Parsons possess three general properties and propensities: "the interdependence of parts or variables . . . , *order* in the relationship among the components . . . [, and] a tendency to self-maintenance, which is generally expressed in the concept of equilibrium." The psychoanalytic principles originally elaborated by Sigmund Freud and increasingly employed by American academics likewise stress the importance of fundamental patterns in human life and the ability of the disciplined mind to comprehend them. There are many schools of psychoanalytic thought, but each posits a relationship between childhood experience and adult action that is not immediately apparent from naive empirical observation. The trained psychoanalyst maintains the validity of the general theoretical system—while attempting to understand its unique manifestation in the particular case—even in the face of the patient's explicit denial of the suggested linkages between present behavior and the emotional trials of early childhood. The *Annalistes* and the Marxists use their social theories in a similar fashion—to probe behind the surface of events to achieve a more convincing explanation of reality than that afforded by an empiricist approach. In the end, each mode of analysis stands or falls by the degree to which it meets the test of the "reality rule," a more con-

[38] Lévi-Strauss, *Structural Anthropology*, 279; and Hall, "Marxism and Culture," *Radical History Review*, 18 (1978): 12.

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vincing Hexter guideline: "the best and most likely story that can be sustained by the relevant extrinsic evidence."[39] This criterion cannot be satisfied simply by ascertaining the "facts," for the point at issue is the explanatory scheme that penetrates to the heart of their reality. This problem demands discussion and debate; it cannot be resolved by the a priori exclusion of Marxism or any other rationalist social theory.

Although the pragmatic tradition has narrowed the critical scope and interpretive range of American historical analysis, it has produced a number of significant methodological approaches to the study of the past. In particular, this empiricist perspective has encouraged the careful examination of the personal experiences of historical actors—their values, goals, and behavior. This focus on the details of existence and people's perception of them constitutes a crucial aspect of the discipline of social history, for it is within the inchoate world of everyday life—the matrix of half-seen and imperfectly grasped objective structures—that men and women seek to interpret reality and to affect it. How do historical actors comprehend the flux of daily existence? How can the observer reconstruct their subjective world-views? These questions move us on to new terrain. The shape of the objective contours of the cultural landscape—its structures, productive modes, and hierarchies of power—diminish in importance. The subjective dimensions of human experience take precedence. Given the complexity of the social world, how do individuals formulate and then seek to attain their goals?

"Most men are philosophers," the Italian Marxist Antonio Gramsci once suggested, " in as much as they operate on the practical level and in their practice (in the controlling pattern of their conduct) have a conception of the world, a philosophy that is implicit." This perceptive remark to the contrary, the systematic examination of the significance of the ordinary behavior of individuals has its strongest theoretical roots not in Marxism but in American pragmatic philosophy. "The meaning of an intellectual conception," Charles S. Peirce insisted, resides in the "practical consequences [that] might conceivably result by necessity from the truth of that conception." The pragmatism of Peirce and of William James embodied a phenomenological theory of human knowledge; as subsequently developed by the German philosopher Edmund Husserl, this epistemology takes as its point of departure the "pure phenomena" of the individual's act of "perceiving, judging, experiencing, and willing." This cognitive approach to human reality finds contemporary expression in the ethnographic methods and goals of many American anthropologists. Two of the central tasks of anthropology, Clifford Geertz has asserted, are the comprehension of the "subjective" mental worlds of the members of a social group and the elucidation of the "system of ideas . . . immanent in every overt process of human action."[40] A similar phenome-

[39] Parsons, *Social System* (New York, 1951), as cited in Berkhofer, *A Behavioral Approach to Historical Analysis*, 183–85; and J. H. Hexter, "The Rhetoric of History," *History and Theory*, 6 (1967): 5, reprinted in his *Doing History* (Bloomington, Ind., 1971), 19.

[40] Gramsci, *Il materialismo storico e la filosofia di Benedetto Croce*, 96, as quoted in Gwynn A. Williams, "Gramsci's Concept of Egenomia," *Journal of the History of Ideas*, 21 (1960): 590, 586–99; Peirce, *Collected Papers of*

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nological perspective informs the historical scholarship of Herbert G. Gutman. Gutman's perceptive and sympathetic studies of American workers and of Afro-Americans are presented, in nearly overwhelming detail, through first-person accounts. Quotations from a myriad of documentary sources—diaries, letters, songs, sermons, speeches, and newspapers—tell the story from the inside, from the perspective of the historical actors. Although he provides a coherent picture of the objective realities of class positions and social authority, Gutman's primary concern is not with structures of power but with the inherent value of the lives of the people who inhabit them. His main goal remains the depiction of the traditions, experiences, and struggles of those who inhabited the past—their own conception of the world in which they lived.[41]

The study of "collective mentality" by the important French social historian Robert Mandrou reflects somewhat similar analytic principles. Mandrou's methodology involves both a close examination of written documents that reflect the actors' conceptions of reality and a careful assessment of the cultural and psychological impact of the objective structures of existence: What states of mind and of feeling, for instance, might appear in a society with a high rate of mortality and a low level of agricultural production? How might these be molded or distorted by the theological doctrines propagated by the dominant religious institutions of the culture? Mandrou's hypotheses are stimulating, in part because of their precise formulation, for he has separated "the elements prevailing throughout French society" during the sixteenth century (hypersensitivity of temperament, social aggressiveness, and a feeling of impotence in relation to the natural world) from the "differential mental structures" of distinct groups; "each social class—as also each profession or even religious group—had its characteristic outlook."[42]

These attempts by cognitive anthropologists and phenomenologically inclined historians to comprehend the consciousness of individuals and social groups are replete with difficulty. One French critic points to the "disconcerting ease" with which historians of *mentalité* move from an analysis of social behavior to the depiction of the ideas or the mental states that behavior is alleged to indicate. These problems are particularly acute with respect to those nonelite members of the population who left few written records. Even the availability of first-hand accounts does not resolve the problem of interpretation, for the historian (unlike the anthropologist) cannot verify a hypothesis by talking to living informants. The "thick description" of a culturally significant episode—the intensive method of presentation that Geertz prescribed for anthropologists and that Gutman intuitively employed—obviates some of these criticisms. Still, im-

Charles Sanders Peirce, ed. Charles Hartshorne and Paul Weiss, 5 (Cambridge, Mass., 1935): 6; Kolakowski, *Husserl and the Search for Certitude*, 37, 69; and Geertz, *The Social History of an Indonesian Town* (Cambridge, Mass., 1965), 141. Also see William James, *Pragmatism and Other Essays*, ed. Joseph L. Blau (New York, 1963), 23–24, 92–93; and Raymond Williams's Introduction to Goldmann, *Racine*, xvi–xviii.

[41] Gutman, *Work, Culture, and Society in Industrializing America: Essays in American Working-Class and Social History* (New York, 1976), and *The Black Family in Slavery and Freedom, 1750–1925* (New York, 1976).

[42] Mandrou, *Introduction to Modern France, 1500–1640*, 236–42.

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portant conceptual problems confront the historian who wishes to present a phenomenological portrait of a complex nontribal society. Nonlocal or specialized institutions (such as a banking system) often cannot be depicted subjectively, because their scope extends beyond the experience of most individuals in a society.[43] This deficiency can be offset, at least in part, by the anthropological technique of using different informants or documentary sources to construct a comprehensive picture of the social world. No individual or group sees the cultural landscape as a whole, but a mosaiclike tableau can be pieced together from the various perspectives.

The innovative scholarship of Clifford Geertz is instructive in this regard. In his *Social History of an Indonesian Town* Geertz constructed a "cultural paradigm," a symbolic structure that encompasses "the main conceptual categories in terms of which the inhabitants of the Modjokuto of 1952–1954 themselves perceived their society—of the principles of social grouping they used, the manner in which they regarded those principles to be interrelated, and the qualitative characteristics in terms of which they assigned concrete individuals to particular groupings." According to Geertz, his description represented a "realistic classification of the primary blocks of the regional social system" and embodied Javanese perceptions. The symbolic model was "phenomenologically real . . . despite the fact that no single Modjokutan could present it . . . as a system."[44] Such cultural paradigms, though created by the anthropologist, are recognized as substantially correct by various members of the society—at least insofar as these models accord with their own experience. By clarifying the prime features of a complex reality, these symbolic syntheses broaden the interpretive scope of a phenomenological approach. The distanced, composite conceptions of the social analyst are very different from those of any single actor, but the vantage point is much the same. The society is presented from the inside, as subjective experience.

Geertz devised a cultural paradigm that was "phenomenologically real" to more than a single individual to avoid the dangers inherent in phenomenology: its subjective emphasis and idealist assumptions. In a less symbolic manner Pierre Bourdieu resolved similar epistemological problems in a fine study of the marriage strategies of the peasants of Béarn, a remote area of the Pyrenees. On the basis of careful documentary research and extensive questioning, the French anthropologist argued that these land-owning families have always acted purposefully "to safeguard the essential at all times" and that their main goals were culturally defined as the maintenance of the family line on an economically viable landed estate. Like the concept of "moral economy" held by the eigh-

[43] Perrot, "The Strengths and Weaknesses of French Social History," 171; Geertz, *The Interpretation of Cultures*, chap 1; Julian H. Steward, *Theory of Cultural Change: The Methodology of Multilinear Evolution* (Urbana, Ill., 1955), 46–55; and Hobsbawm, "From Social History to the History of Society," 16.

[44] Geertz, *The Social History of an Indonesian Town*, 121, 141, 119–50 in general. For a philosophical explication and defense of this approach, see Peter L. Berger and Thomas Luckmann, *The Social Construction of Reality: A Treatise in the Sociology of Knowledge* (New York, 1966). And, for two attempts to relate these theoretical premises to historical study, see Richard R. Beeman, "Community in Colonial America," *American Quarterly*, 29 (1977): 422–43; and Harry S. Stout, "Culture, Structure, and the 'New' History: A Critique and an Agenda," *Computers and the Humanities*, 9 (1975): 216–29.

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teenth-century English laborers whom Thompson studied, these commitments to the lineage and the patrimony reflected long-standing values, "the whole system of predispositions inculcated by the material circumstances of life and by family upbringing." Parents undertook a "concerted effort at indoctrination and cultural reproduction" so that the children might understand that "the land belongs to the eldest son and that the eldest son belongs to the land."[45] To those born into this rural culture, the principle of primogeniture was experienced as an orally transmitted value and as a visually perceived occurrence. By precept and by example each adult generation taught its successor that the land inherits its heir.

If the objectives were clear, the process by which they were realized was not. Each peasant family was locked in a situation that it could affect but not control. Parents confronted the vagaries of fertility and mortality; they could not ensure that the first- or second-born child would be male, would survive the diseases of childhood, or would have the requisite physical strength and financial cunning to manage the property. Nor could they determine the level of prices for their agricultural products or the regional demand for wage labor. Most important of all, the parents could not affect the availability of suitable marriage partners for their children. Yet all of these factors (as well as their own health) influenced their ability to achieve their goals. Because reality was so complex and unpredictable, these peasants had no recourse to fixed or immutable rules of conduct—to a blind implementation of legal rules or cultural norms governing the inheritance of property. Were they therefore completely at the mercy of the accidents of fertility and translocal historical forces? Were their lives bereft of personal control and cultural direction?

The implications of Bourdieu's answers to these questions transcend the substantive points at issue, for his analysis demonstrates the existence of purposeful action within the bounds of rigid cultural structures. The decisions made by the peasants of Béarn, Bourdieu has suggested, indicate that they had a "system of schemes" for achieving their goals amid the contingencies of the world. Their marriage priorities were crucial in this respect. Their strategies were not explicitly or coherently articulated, but they can be seen upon close inspection to have been "rooted in a small number of implicit principles": the primacy, most importantly, of men over women and of the eldest over all younger siblings. Thus, younger sons were often discouraged from marrying so that their labor could be utilized on the family holding. A family overburdened with daughters usually attempted to marry them off to less affluent partners, in order to minimize the amount of the dowry (*adot*). Conversely, parents contemplated a socially "upward" marriage for the eldest son but consummated it only under certain circumstances—the arrival of a high-status bride sometimes threatened the "principle of male pre-eminence" within the household and endangered the standing

[45] Bourdieu, "Marriage Strategies as Strategies of Social Reproduction," in Robert Forster and Orest Ranum, eds., *Family and Society: Selections from* Annales (Baltimore, 1975), 119, 118, 129–30. For the explication of a philosophical position very similar to that underlying Bourdieu's analysis, see James Miller, "Merleau-Ponty's Marxism: Between Phenomenology and the Hegelian Absolute," *History and Theory*, 15 (1976): 109–31.

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of the male lineage in the community. The advantageous marriage of a son also increased the risk of subsequent disaster, since the dowry had to be repaid in full if the husband died before an heir was born. Given the wide variety of circumstances and considerations, it is impossible for the outside observer to predict how a given family would act on a particular occasion. Yet the decisions of these rural cultivators had an internal logic when seen in relation to "the entire *matrimonial history* of the family." Seen from the inside—from a phenomenological perspective—their strategies were "the product of *habitus*, meaning the practical mastery of a small number of implicit principles that have spawned an infinite number of practices."[46]

This conceptualization constitutes an important contribution to the definition of the process of social persistence and social change. For Bourdieu, *habitus* means an action-oriented world-view that furnishes "the basis for the casuistic thinking required to safeguard the essential at all times." It comprises both the productive mode of this French rural culture and the flexible and contradiction-ridden process by which it has been perpetuated and altered. Since the peasant's *mentalité* has been "the product of the very structures" it has tended to reproduce, it has embodied elements of stasis and tradition. A dynamic, creative aspect has also been present, for the hard-pressed cultivator has continually had to resolve the tensions created by changing social and economic circumstances.[47] Like the young, nineteenth-century rural women studied by Louise A. Tilly and Joan Scott (girls who took jobs in urban and industrial settings in order to continue to contribute to the traditional "family economy"), Bourdieu's peasants were neither prisoners of the past nor masters of the contemporary environment. Rather, they have behaved as active agents seeking by one means or another to cope with an emergent reality. Their goals have been at least partially manifested in their rules of behavior, their pragmatic "system of schemes." This matrix of strategies has constituted their "implicit philosophy" in Gramsci's sense of the term. The peasants' world-view has been at once their conception of life and the system of social and productive relationships that have sustained it, a *Weltanschauung* anchored in reality and phenomenologically real to the actors.[48]

For American social historians, Bourdieu's conceptualization of human agency and social process represents an attractive synthesis of the dominant pragmatic and voluntarist tradition of their native culture and the rationalist European intellectual systems of structuralism and Marxism. The limiting structures of the inherited past—its settled institutions, habitual practices, and constricting ideologies—form an integral part of the analytic framework, but these structures do not predominate. Equal importance is accorded to the con-

[46] Bourdieu, "Marriage Strategies as Strategies of Social Reproduction," 119, 134, 136, 141, *passim*.

[47] *Ibid.*, 119, 141. Compare Bourdieu's argument with that advanced by the intellectual historian J. G. A. Pocock: "Every society possesses a philosophy of history—a set of ideas about what happens, what can be known and done . . . which is intimately a part of its consciousness and functioning. . . . The concepts which we form from, and feed back into, tradition have the capacity to modify the content and character of the tradition conceptualized . . ."; Pocock, *Politics, Language, and Time: Essays on Political Thought and History* (New York, 1971), 233, 235.

[48] Tilly and Scott, "Women's Life and Work in Nineteenth-Century Europe," *Comparative Studies in Society and History*, 17 (1975): 36–64.

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flicts inherent in the modes of social reproduction and the dialectical process by which they are resolved. Finally, these objective structures and conflicts are subordinated to the subjective experiences of the historical actors; their "life-worlds" stand in the foreground. Bourdieu's phenomenological approach demonstrates the ability of men and women to shape their own lives, however narrow and restrictive the bounds of the cultural environment. This stress on human agency among modest tillers of the soil should have an especial appeal to historians in the United States. In a society formally committed to the democratic ideals of liberty and equality, the ultimate test of a historical method must be its capacity to depict the experiences of all members of the culture and to comprehend the ability of all individuals in the culture to make their own history.[49]

WHAT TYPE OF LITERARY PRESENTATION is implied by a methodology that seeks to determine the objective structures of existence, to define the hierarchical relations among them, and to depict the experiences of historical actors from a phenomenological perspective? Both the mode of argument in a work of history and its prose style affect the substantive content and thereby influence its intellectual or emotional impression on the reader. Carl Schorske has called attention to "the impact of a book's mental structure . . . , its grammar—the way it gives plausibility to the empirical materials and the tightness of its articulation." Like any other form of written endeavor, a social history must attain the appropriate synthesis of substance, argument, and style. The rhetoric of a work of scholarship, for example, often reflects the methodological preferences of the author. "A social structure can be described without any reference whatsoever to particular individuals and without the use of specific cases," F. G. Bailey has noted in this regard, and "a description of this kind, if well done, makes a strong appeal to the intellect; it is elegant, sparing in detail, rounded and complete, and has a sharp, tidy, thoroughly 'scientific' exactness." Bailey's objection to this "work of logic" proceeds less from esthetic than from interpretive principles; such neat and self-contained models of social equilibrium cannot easily explain "the dynamic aspects of social life: growth, conflict and transformation."[50] An equally serious criticism relates to the reductive features of such an account. By systematically excluding the ambiguities of individual existence, it fails to convey the full emotional complexity of the historical situation. What is the value of a social history that primarily demonstrates the conceptual skills of the author and eliminates the contradictions inherent in the lives of the actors—many of which can be depicted only by a more biographical form of presentation?

This question raises important issues of method and is best approached

[49] In following Jesse Lemisch's admonition to "study the conduct and ideology of the people on the bottom" of society and thus to "make the inarticulate speak," it is also crucial to heed his advice to "continue to examine the elite" in order to assess its critical role in the historical process; "The American Revolution Seen from the Bottom Up," 6.

[50] Schorske, as quoted in "New Trends in History," 955; and Bailey, *Tribe, Caste, and Nation*, 14.

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through a consideration of the epistemological categories devised by Louis O. Mink. Mink has distinguished three "modes of comprehension" by which the human mind orders and assimilates the "data of sensation, memory, and imagination": the theoretical, the categoreal, and the configurational.[51] These abstract terms, which Mink derived from the disciplines of mathematics, philosophy, and literature, refer in effect to three distinct approaches to the writing of social history. These divisions are similar to, but not identical with, the three paths taken by the practitioners of the "new social history" in the United States. Some authors employ quantitative techniques, seeking—in Mink's terms—a theoretical understanding of human reality. Others instinctively conceptualize their data, creating categories of social groups and types of historical behavior. Finally, still other historians rely on a narrative framework as a prime analytic device, assuming that meaning will emerge from a close description of the chronological process. Each of these modes of understanding—quantitative, conceptual, and narrative—has certain strengths and weaknesses that, upon examination, reveal the necessity of a composite rhetorical mode of presentation, an "action model," that offers one means of reconciling the competing organizational priorities of chronology and analysis. This "action model" also addresses the relationship between the rhetoric of a work of history and its interpretive assumptions. In a manner similar to Bourdieu's concept of *habitus*, the "action model" focuses attention on the importance of human agency even as it depicts the limiting forms, structures, and geographies of the historical context.

Quantification holds great appeal for many historians working in the American pragmatic tradition because of its resemblance to the empiricism of nineteenth-century science. The analysis of empirical data with statistical methods supposedly produces results that are precise and value free. Neither assumption is completely warranted. Quantification achieves incontestable interpretive conclusions only in restricted areas of research—those dealing with established institutional systems, clearly defined rules of behavior, or a small number of discrete variables. Legislative voting has been analyzed with considerable success because of its narrow context, while broader studies of the socioeconomic correlates of membership in a political party have been much less convincing. A second methodological shortcoming is equally important. Despite its apparently scientific character, quantification does not necessarily yield an objective view of human reality. Consider the study of "social mobility," an area of intense research in the United States during the past fifteen years.[52] The quest for home ownership by working-class immigrant families has not been interpreted, for the most part, in "traditionalist" terms—as the parents' attempt to provide security for themselves during old age or to root the old lineage firmly in the soil of the

[51] Mink, "History and Fiction as Modes of Comprehension," *New Literary History*, 1 (1969–70): 547–49, *passim*. The following argument relies heavily on the categories and reasoning expounded in this fine article.

[52] See, for example, Jackson T. Main, *The Social Structure of Revolutionary America* (Princeton, 1965); Stephan Thernstrom, *The Other Bostonians: Poverty and Progress in the American Metropolis, 1880–1970* (Cambridge, Mass., 1973), and *Poverty and Progress: Social Mobility in a Nineteenth-Century City*; and Edward Pessen, *Riches, Class, and Power before the Civil War* (Lexington, Mass., 1973), and Pessen, ed., *Three Centuries of Social Mobility in America* (Lexington, Mass., 1974).

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New World. Rather, the purchase of a house has usually been viewed from a "liberal" perspective, as the achievement of the individual male laborer in his effort to "get ahead." Since both interpretations are consistent with much of the quantitative evidence, the perspective of the author has determined the thrust of the argument, not the evidentiary base.

The inherent demands of the statistical method for numerically comparable data has also distorted the immigrant experience. Quantitative historians have measured "success" according to performance on a uniform scale of economic achievement and not in terms of the cultural values and social priorities of specific ethnic groups. Many Irish laborers, for example, did not "want" to invest their hard-earned dollars in small businesses, the extensive education of their children, or the acquisition of new occupational skills. They "preferred" to support the Roman Catholic Church and to achieve the security of home ownership. A mere statistical comparison of their occupational or economic success with that of the members of other ethnic groups (with different priorities) distorts the significance of their lives. The very features that make the quantitative mode of understanding such a powerful analytic tool—its empiricism, narrow focus, and statistical precision—serve to conceal the phenomenological essence of the life-worlds of European, Asian, and Afro-American migrants to the industrial cities of the United States.[53]

Aware of these limitations, other American social historians organize their research in a conceptual manner and thus exemplify the second of Mink's formulations. For purposes of presentation, these scholars simplify reality by placing individuals and events that have similar features into a distinct category. This approach encourages a holistic, contextual analysis of social groups. Thus, many historians of colonial American communities study families in generational cohorts. Their investigations admit diversity within a given chronological period but stress the crucial importance of a common historical experience. The range of family behavior (with respect to age at marriage, number of children, and pattern of inheritance) within the same generation appears to be less significant than the differences between successive groups of parents.[54] A similar analytic principle underlies Peter Loewenberg's innovative study of the emotional and nutritive deprivations produced by World War I on a generation of German infants, the cohort of young men and women who subsequently came of age during Adolph Hitler's rise to power. Loewenberg's conceptual approach provides great interpretive leverage. Viewing the decisions of young people to support the authoritarian government of the Third Reich from a generational perspective,

[53] James A. Henretta, "The Study of Social Mobility: Ideological Assumptions and Conceptual Bias," *Labor History*, 18 (1977): 166–78; Daniel D. Luria, "Wealth, Capital, and Power: The Social Meaning of Home Ownership," *Journal of Interdisciplinary History*, 7 (1976): 262–82; and William H. Sewell, Jr., "Social Mobility in a Nineteenth-Century European City: Some Findings and Implications," *ibid.*, 217–33.

[54] See, for example, Greven, *Four Generations: Population, Land, and Family in Colonial Andover, Massachusetts*; Daniel Scott Smith, "The Demographic History of Colonial New England," *Journal of Economic History*, 32 (1972): 165–83; Robert V. Wells, "Family Size and Fertility Control in Eighteenth-Century America: A Study of Quaker Families," *Population Studies*, 25 (1971): 73–82; Nancy Osterud and John Fulton, "Family Limitation and the Age at Marriage: Fertility Decline in Sturbridge, Massachusetts, 1730–1850," *ibid.*, 30 (1976): 481–94; and Kenneth A. Lockridge and Alan Krieder, "The Evolution of Massachusetts Town Government, 1640 to 1740," *William and Mary Quarterly*, 3d ser., 23 (1968): 549–74.

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he has suggested that these choices had a common psychological origin in the experiences of children during the wartime years. Like the historians of *mentalité*, Loewenberg constructed a conceptual model that attempted to demonstrate a collective psychobiographical pattern where others had seen only accident and variety.[55]

Although this conceptual form of presentation is ideal for the depiction of the composite "mind" of the elite of an age (as the impressive synthetic creations of Perry Miller in *The New England Mind* and Henry Nash Smith in *Virgin Land* attest), it does not permit the specification of precise causal relationships or the delineation of unique historical events.[56] These deficiencies proceed directly from the method, for individual cases are subsumed within or subordinated to a controlling conceptual scheme. The historian creates a coherent mosaic from disparate materials, a pointillist technique applicable to social as well as intellectual history. E. P. Thompson's articles on food riots, work-discipline, and plebian culture in eighteenth-century England are cases in point. Thompson has presented a great quantity of evidence drawn from primary documents that suggests the consciousness of the historical actors, but his conceptual approach makes it difficult to trace an actual series of events in a specific locality. Examples of the social conflicts generated by the transition from the traditional system of food distribution to the new political economy of agricultural capitalism are drawn from widely separated regions and from different decades. Thompson has juxtaposed a food riot in Norwich in 1740 with one in Gloucestershire in 1766; the "just price" demanded by a hungry crowd in the Isle of Ely receives articulate justification in an edict propounded by a paternalistic magistrate in Middlesex and an anonymous "Clergyman in the Countryside"; and a passage from *The Wealth of Nations* defends the proposition that the calculus of the market distributes grain in the most socially efficient manner. Thompson has used a conceptual mode of presentation to isolate the historic groups, economic forces, and cultural values present in England during the extended transition to capitalism, and he has then set them in conflict with one another—collapsing chronological time and ignoring geographical distance for purposes of drama and argument.[57] This adept organization overcomes the inherent limitations of the conceptual mode of comprehension with respect to specificity and causality, but only by a deft sleight of hand. The exact process of capitalist transformation—as it actually occurred in a circumscribed social context, with

[55] Loewenberg, "The Psychohistorical Origins of the Nazi Youth Cohort," *AHR*, 76 (1971): 1457–1502. For a good general survey of the literature, see Alan B. Spitzer, "The Historical Problem of Generations," *ibid.*, 78 (1973): 1353–85.

[56] Miller's first volume was published in 1939 and was based on the writings of only sixty colonial authors, six of whom accounted for 56 percent of Miller's documentation; Miller, *The New England Mind: The Seventeenth Century* (New York, 1939); and George Selement, "Perry Miller: A Note on His Sources in *The New England Mind: The Seventeenth Century*," *William and Mary Quarterly*, 3d ser., 31 (1974): 453–64. The later work of Smith has a more symbolic approach but is also restricted to the literary elite; Smith, *Virgin Land: The American West as Symbol and Myth* (Cambridge, Mass., 1950); and Rush Welter, "The History of Ideas in America: An Essay in Redefinition," *Journal of American History*, 51 (1965): 599–614.

[57] Thompson, "The Moral Economy of the Crowd in Eighteenth-Century England," 89–91, 97, 110–11, 133, *passim* Also see Thompson, "Time, Work-Discipline, and Industrial Capitalism," *Past & Present*, no. 38 (1967): 56–97, and "Patrician Society, Plebian Culture," 382–405.

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the causal links palpably manifest (and thus "verified") in individual lives—escapes confirmation.

Yet this type of historical account, the presentation of identifiable actors in a distinct spatial-temporal setting, constitutes the most familiar form of historical writing. This narrative tradition falls within Mink's third mode of understanding: the configurational method of "grasping together in a single mental act" various diverse events; and Mink has attempted to demonstrate its explanatory power and its utility in historical analysis. Narratives, he has maintained, "are not imperfect substitutes for more sophisticated forms of explanation and understanding, . . . the unreflective first steps along the road which leads toward the goal of scientific or philosophical knowledge." The comprehension at which narratives aim represents "a primary act of mind," he has argued; both from history and from fiction "we learn how to tell and to understand complex stories" and "how it is that stories answer questions." For example, great difficulties attend the analysis of the exact "combination of motives, pressures, promises and principles which explain a Senator's vote." To apprehend "the tableau of objects in their concrete particularity as well as in their manifold relations" requires a complex act of mind; all of the diverse contexts and sequences of action must be connected in a convincing manner "by a network of overlapping descriptions."[58] Most events—as psychohistorians often point out—are "over-determined," the product of the convergence of a multiplicity of personal and circumstantial causes; thus, it requires sharp analytic and synthetic skills to construct a convincing "story." To compose a coherent and compelling narrative is to engage in a high-order intellectual activity with stringent standards of excellence.

Nonetheless, many social historians remain skeptical of the interpretive range and power of the narrative mode of presentation. Traditional chronological accounts are often restricted in scope to public affairs and to the lives of the elite. Moreover, narrative history is often impressionistic in its use of evidence, theoretically diffuse, and—because of its dependence on literary documents—ill-equipped to grapple with the wide variety of important historical problems amenable to quantitative or conceptual types of analysis. The validity of these criticisms may be readily granted without negating the very real utility of the narrative mode in the writing of social history. This approach directly addresses questions of time, change, and sequence; such chronological concerns, as the scholarship of Marxists and the early *Annalistes* acknowledge, form crucial aspects of the discipline of history. Narratives also embody a phenomenological perspective, and for that reason have been widely adopted by historians working in the pragmatic tradition. By placing as much (or more) emphasis upon the subjective perceptions of the actors as upon the objective circumstances of existence, narratives underscore the importance of human agency.[59] Finally, histo-

[58] Mink, "History and Fiction as Modes of Comprehension," 547, 556–58. For a stimulating defense of the narrative tradition, see Vann, "The Rhetoric of Social History," 221–36.

[59] Although some degree of artifice enters into the construction of a narrative—in that the author already knows the outcome of the story and therefore provides a false sense of open-endedness–artifice alone does not

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rians who adopt a chronological framework establish a basic congruence between the lives of their subjects and those of their audience. Most readers view the past in the same manner that they comprehend their own existence—not in quantitative or conceptual terms but in terms of a series of overlapping and interwoven narrative life-stories. Narrative history holds great appeal for the lay reader not primarily because of the absence of "jargon" but because its mode of cognition approximates the reality of everyday life.

The task is clear. If American social historians are to reach a wide audience, then they must fashion a rhetorical mode of presentation that reconciles narration and analysis. Impressive advances have already been made in two grandly conceived studies of Afro-American society during the nineteenth century. In *The Black Family in Slavery and Freedom* Herbert Gutman has demonstrated the evocative power of a phenomenological depiction of the Afro-American experience and, through the pragmatic application of anthropological methods, has interpreted this experience in terms of a wider theoretical framework. Different analytic priorities inform the scholarship of Eugene Genovese. While his *Roll, Jordan, Roll: The World the Slaves Made* also describes the world-views of various groups within the black population, Genovese has subsumed these subjective perspectives within a Marxist structural analysis of southern slave society and a dialectical model of the development of black (and white) consciousness. His rationalist and structural approach stands in marked contrast to the pragmatic and phenomenological assumptions underlying Gutman's work.[60]

For a synthesis of these contrasting modes of analysis and their integration with a chronological narrative, less sweeping examples of historical scholarship are more instructive. The fine book by Paul Boyer and Stephen Nissenbaum, *Salem Possessed: The Social Origins of Witchcraft*, serves as a fine example. *Salem Possessed* begins with a short narrative account of "What Happened in 1692," the judicially sanctioned murder of thirteen women and six men for the practice of witchcraft. This moment in American history remains compelling, and the authors have consciously exploited its hold on the public imagination. Like many writers of fiction, they employed the device of the "symbolic moment," using the "interaction of the two—the 'ordinary' history and the extraordinary moment—to understand the epoch which produced them both."[61] Michael Katz has utilized a similar strategy (with respect to less familiar and less dramatic materials) in his stimulating examination of early school reform in nineteenth-century Massachusetts. "This study," Katz explained, "focuses on small, concrete situations, which it tries to examine thoroughly." Intuitively adopting the technique of "thick description" advocated by Geertz, Katz isolated key paradigmatic events—such as the rejection, by working-class voters in Beverly, Massachusetts, of a plan for a publicly supported high school—and assessed their wider social

constitute a major objection. For the reader's imperfect knowledge approximates that of the historical actor; neither knows for certain how events will turn out.

[60] For a statement of these differences in a strident form, see Elizabeth Fox-Genovese and Eugene D. Genovese, "The Political Crisis of Social History: A Marxian Perspective," *Journal of Social History*, 10 (1976): 210–17.

[61] Boyer and Nissenbaum, *Salem Possessed: The Social Origins of Witchcraft* (Cambridge, Mass., 1974), xii.

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importance. His technique was "to start with the concrete and through careful analysis to work outward to conclusions of broad cultural significance."[62]

These dramatic devices have important methodological implications. In scholarly terms, the authors of both works have defined a *histoire problème*, a small but crucial event that manifests many of the major social forces and intellectual currents extant in the larger society. The successful emotional depiction and analytical resolution of this limited but nonetheless complex causal puzzle provides an elegant answer to the organizational problem defined by Braudel—a method that would integrate "event, conjuncture, and structure."[63] This conceptual approach can be widely utilized; even those scholars addressing broad, open-ended processes of cultural change can focus parts of their account around particularly instructive episodes.

Salem Possessed offers another rhetorical strategy of considerable utility to social historians, for it demonstrates how quantitative data and conceptual models can be presented as a series of interrelated problems or arguments within a chronological and narrative framework. Early in their account Boyer and Nissenbaum posed the question of the geographical relationship between the alleged witches and their accusers. According to the maps drawn by the authors, the houses of the two groups were not distributed in a random fashion across the landscape. Rather, the locations of their residences conformed to a wider sectional and religious division within the community. The alleged witches were tied by propinquity and financial interest to Salem Town, a diversifying commercial seaport, while their accusers lived predominantly in the agricultural region known as Salem Village. As important as the substance of this argument are the explicit terms in which it is made. Throughout *Salem Possessed* Boyer and Nissenbaum have addressed methodological questions in the text itself. The narrative that opens the study is gradually transformed into an intricate exploration of empirical evidence and analytic methods, thus expanding the cognitive skills of the readers while deepening their understanding of the historical context. Although the organization remains chronological in form, based on interwoven stories relating to specific lives and events, a "Collective Profile" illuminates the common features in the lives of the condemned witches and statistical tables chart the correlations among the variables of wealth, church membership, and factional alignment.[64] Yet these structural and statistical analyses (as well as psychobiographies of the leading protagonists and a textual exegesis of the minister's sermons) are consistently subordinated to the two main narrative themes: the transition from subsistence to commercial production in Salem during the late seventeenth century and the chains of causation linking the individuals caught in this economic transformation to the tragic events of 1692.

This astute resolution of the problems involved in combining Mink's three modes of comprehension represents a significant achievement. When Clifford

[62] Katz, *The Irony of Early School Reform: Educational Innovation in Mid-Nineteenth-Century Massachusetts* (Cambridge, Mass., 1968), 14–15.

[63] Hexter, "Fernand Braudel and the *Monde Braudellien*," 537–38.

[64] For the geographical analysis, see Boyer and Nissenbaum, *Salem Possessed: The Social Origins of Witchcraft*, 33–35, and, for the "Collective Profile," see *ibid.*, 190–201. The book contains seven maps and six tables.

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Geertz published his *Social History of an Indonesian Town* in 1965, he suggested that narration and analysis were contradictory rather than complementary. His book, therefore, constituted "an attempt not so much to re-create the past as to discover its sociological character." As Geertz took pains to point out, his account is "not a story and so has neither moral nor plot"; rather, "it is a theoretically controlled analysis of certain processes of social change and contains instead an argument."[65] Both Boyer and Nissenbaum's *Salem Possessed* and Katz's *The Irony of Early School Reform* transcend this formulation; for they illustrate the possibility of simultaneously introducing characters, chronology, and a matrix of action on the one hand and diverse analytic methods on the other. In the case of *Salem Possessed*, argument and plot converge—literally and symbolically—in Boyer and Nissenbaum's contention that the witchcraft hangings can ultimately be traced back to a conflict between religious factions "in which a subsistence, peasant-based economy was being subverted by mercantile capitalism."[66] Although the authors fail to demonstrate their argument in a completely convincing fashion (minimizing the autonomy of religious world-views and neglecting to prove that the members of the two church factions employed different modes of production), they have fashioned a powerful organizational method. Their synthesis, like the epistemological formulation by Pierre Bourdieu, represents an important contribution to the theory of the "new social history."

INDEED, WHEN TAKEN TOGETHER, the methodological innovations of Bourdieu and of Boyer and Nissembaum suggest a composite analytic and rhetorical strategy, an "action model" designed to discover and depict history as it was lived by men and women in the past. The implementation of this model demands, in the first place, the definition of a *histoire problème*, a focused paradigmatic episode, the resolution of which speaks to the validity of a social theory or to an important question of historical interpretation or cultural significance. The direct specification in the text of moral and methodological problems constitutes a second aspect of this approach, for an avowed goal is the development of the critical and cognitive consciousness of the audience. As Paul Hernadi has argued, the contemporary historian should not pretend "to *mirror* what really happened" but should "freely admit that he mediates." The responsibility of the scholar is, in fact, "to make us see the past from within and from without at the same time—as evolving drama and as the fixed target of distanced retrospection."[67] A method for achieving this dual objective makes up the third characteristic of the "action model." The quantitative techniques and structural approaches of *Annalistes*, Marxists, and social scientific historians are used to delineate the objective character of the society and to determine its hierarchial systems.

[65] Geertz, *The Social History of an Indonesian Town*, 2.
[66] Boyer and Nissenbaum, *Salem Possessed: The Social Origins of Witchcraft*, 178, xii.
[67] Hernadi, "Re-Presenting the Past: A Note on Narrative Historiography and Historical Drama," *History and Theory*, 15 (1976): 45, 47.

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Simultaneously, a narrative, chronological framework of organization facilitates the presentation of the world-views of the actors themselves, a phenomenological perspective drawn from American pragmatic philosophy and cultural anthropology. The "actions" of individuals—their emotions, their values, and their behavior—remain the ultimate point of reference and give the "model" its name. A social history written in terms of these three postulates focuses narrowly but interprets broadly, critically surveys the past with reference to the present, and, most important of all, records the paradoxical and even tragic history of human agency—of "people acting yet acted upon by the structures of their own history."[68]

[68] The quotation is from an essay on American social history written by Michael Frisch; it will appear shortly in *History and Theory*.

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Comments:

Despite the vagueness of the phrase "new social history," I take myself to be a scholar of such a stripe. "Social history" is itself classically vague, of course, but I think I meet the very broadest criterion—in writing, I tend to leave out the politics. And I tend to use research tools unknown to historians a decade or so ago, hence by definition new, things like the computer, statistical inference, and model testing. Considering myself a "new social historian," I take James A. Henretta's essay as directed to me personally and I am prompted to reply personally, even to the extent of heading these comments "Dear James."

IF I READ YOU CORRECTLY, James, the gist of your critique is that we, as social historians, are a disparate lot. We do not form "a coherent subdiscipline" that you can put your finger on, but are instead "a congeries of groups," often complementary in our approaches, sometimes contradictory. We tend to talk to, and write for, each other, although parenthetically you neglect to point out that this in a measure was forced upon us by early rejection. You might recall J. H. Hexter's vicious attack upon quantification, or the academic loneliness described by Melvin Richter in 1970, the "hostility . . . or what was worse, indifference" of colleagues to one's interdisciplinary work.[1] Finally, you seem to complain, at least by implication, that we have no common philosophy of historical analysis. I would go farther and say that most of our work is done in the absence of any philosophic consideration at all. A few work with some sort of grand scheme in mind. I suspect this to be true of you. But most work because they are simply intrigued with a particular problem, many because they are intrigued by the new tools themselves, and, let us be completely candid, some work only for tenure.

I think, too, that I have a grasp of your corrective, although I must admit to a certain impatience with the discursive nature of your essay and may very well do you an injustice in trying to cut through to the nub of the argument. You introduce the *Annalistes* and English Marxists in order to demonstrate that "coherent intellectual systems of historical inquiry" are indeed feasible. Granted. But neither of these systems can serve us. You point out that the essential pessimism about the human condition of the *Annalistes* fits the French historical experience but runs contrary to American optimism. The English Marxists are more promising as a role model, concerned as they are with the

[1] Hexter to William O. Aydelotte, January 19, 1970, as quoted in Darrett B. Rutman, "Notes to the Underground: Historiography," *Journal of Interdisciplinary History*, 3 (1972): 379; and Richter, ed., *Essays in Theory and History: An Approach to the Social Sciences* (Cambridge, Mass., 1970), 21.

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dialectics of change and optimistic that "the world can be transformed by purposeful action." American liberalism, however, rejects the Marxist model out of hand, and you are enough of a pragmatic American to accept that rejection—albeit that I think I sense a small tear of regret on your part.

Still, you argue, if we can borrow neither system *in toto*, we ought to borrow parts. From both, I gather, we ought to take greater faith in our rational abilities, specifically, borrow the art of using social theories to probe behind those mere facts that have so concerned us as heirs of a pragmatic American tradition. From the *Annalistes* we need to take a concern for the structural limitations on human activity, while from the Marxists we ought to borrow a sense of moral purpose (but directed toward what end?) and the optimistic view that men and women can direct their own lives and, within limits, exceed limitations. Finally, all of these borrowings ought to be merged with the American proclivity for narrative to the end that each of our works of history revolves around some well-defined *histoire problème*; for, after all, as you put it, the clear task is for American social historians to reach a wide audience, and our audience likes narrative.

As a strategy for *your* next book, James, I think all of what you say is grand. I take issue only with your attempt to inflict this singular strategy upon the rest of us. A few years ago, at an American Historical Association session, Robert F. Berkhofer, Jr., pointed to the anarchy prevalent throughout American historiography, devoid as it was (and still is) of any overarching Turnerian or Progressive or what-have-you theme, and suggested a model of American development that could conceivably bring this anarchy under control. My response as commentator was simply, "What's wrong with anarchy?" Insofar as I am concerned, ruling paradigms too rapidly evolve into cant, and I revel too much in the relative freedom from cant, a freedom that the various historiographic "revolutions" of the past years have produced, to second any motion to reintroduce cant. My initial response to you is exactly the same. What is wrong with the fact that we are a disparate lot? Are we not learning a variety of new things by virtue of the multiplicity of our approaches? Would you stay a Daniel Scott Smith because his sometimes esoteric demographics do not fit into "a coherent subdiscipline" of social history or because they cannot be read by a wide audience? You might well answer that I am proceeding in this rejection from my own commitment to empiricism, and you would be right. But I put it to you that you can elevate the power of human reason to whatever high place you choose, exalt to the skies our critical function, yet your rational philosophies will get you nowhere unless you have empirical data to be rational about!

Equally to the point in my rejection, however, is your failure to acknowledge that there are different levels of research. I make the point in terms of a concrete example, again from demographics. To ask the question, "What was the life expectancy of individuals living in the seventeenth-century Chesapeake region?" is to operate at one level. Such a question can be explored and an answer presented without reference to anything outside of the question itself, and the answer will have a distinct value. A different level is in-

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volved in the question, "What relationship did such a life expectancy have to family structure?" and, again, question and answer will have a value distinct to themselves. Still another level obtains in the question, "What relationship did family structure have to the American Revolution?" As I read you, you would have us all, and always, working on this last level. The prescription is wrong on at least two counts.

First, our craft is properly cumulative and cooperative. Viewed one way, my progression of questions forms an ascending rank order, the answer to the last depending upon the answer to the second, which is itself an outgrowth of the first. The scholar working in your ideal genre—that is, at the third level—must do all the work, for you have left no room for scholars to work on the "lesser" levels, no room for cumulation and cooperation. I suggest, too, that, when such a scholar completes the work, it will be so idiosyncratic and subjective as to defy cumulation on even higher levels. Carl Sagan quoting D. H. Lawrence on the nature of the moon is illustrative in this regard: "It's no use telling me it's a dead rock in the sky! I *know* it's not."[2] Well, there is a finite way to demonstrate and demonstrate again that the moon is, indeed, a dead rock, and there are finite ways to build upon that knowledge. There is no way to demonstrate that it is whatever Lawrence thought it was, and no place to go from his subjective musings.

Viewed another way, however, my three research questions form a *de*scending rank order. Again, let us be candid and admit in this forum that the higher the level of abstraction, the easier it is to wax eloquent and the harder it is to test the generalizations expressed. In other words, the rules of the game of high generalization (the historian's favorite game, it seems to me) are so loose that anyone can play and win at least a few adherents. Conversely, the rules of the game at the lower levels are tight, even restrictive, on the one hand but, on the other, offer firm guidelines for acceptance or rejection of the conclusions presented. Viewed in this way, my three questions pinpoint the second fallacy in your prescription. You urge us to rush into easy generalizations at the higher level before we have really learned to live with the new and difficult lower-level tools at our disposal.

I AM NOT AT ALL SURE, James, that this last will cut much ice with you; you have elsewhere expressed your dismay at the "quest for technical exactness and statistical finesse."[3] Hence, I will simply state it as the grounds for my own discomfort with the condition of the new social history—in direct apposition to yours—and leave the matter. It is not the lack of generalization and overweening concern for facts that offends me but the too cavalier attitude toward the rules by which (in these new games of ours) the most probable realities are to be established and the too precipitous rush to grand generalizations so much in evidence. A recent, highly statistical article exemplifies both ten-

[2] Sagan, *The Dragons of Eden: Speculations on the Evolution of Human Intelligence* (New York, 1977), 192.

[3] Henretta, "The Study of Social Mobility: Ideological Assumptions and Conceptual Bias," *Labor History*, 18 (1977): 167.

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dencies. The piece in question reports an examination of eighteenth-century data on marriage and birth from two Virginia counties, places the findings in opposition to Edward Shorter's sexual revolution thesis, then rushes to high generalization: "Industrialization is not necessarily a prerequisite for sexual revolution."[4] On the way to this grand conclusion, the author casually ignores one rule after another of statistical inference. His research plan required correlating tobacco prices and various measures of premarital sex. But he neglected to ensure that his price index would reflect a single marketplace. (It does not; one of his counties is devoted to Oronoco tobacco, the other sweetscented—two varieties with quite different market histories during the century.) And he failed to test for statistical significance. (Of seven leading correlations involving his own price index, four prove to be statistically insignificant; when a more appropriate price index is introduced, no correlation is such as to allow any inference as to the reality whatsoever.) From *your* hands, James, I might well accept a work done according to the model you present. But casting all of our studies in your terms would I fear, with good reason, be disastrous.

DARRETT B. RUTMAN
University of New Hampshire

[4] Lee A. Gladwin, "Tobacco and Sex: Some Factors Affecting Non-Marital Sexual Behavior in Colonial Virginia," *Journal of Social History*, 12 (1978): 72, 57–75.

THE CHIEF SIGNIFICANCE OF THIS ESSAY lies less, in my opinion, in James A. Henretta's efforts to solve some of the knotty problems of contemporary historiography, important as this aim is, than in what his solutions tell us about the state of the American historical profession today. According to Professor Henretta, social history is more than just another professional specialty; it is rather, to use his quotation from Harold S. Perkin, "all of history from the social point of view." Such a claim announces the ascendancy of a new paradigm in American historiography to replace the older, so-called consensus interpretation of the American past. If the older interpretation emphasized ideas and made, implicitly at least, the concept of culture central to understanding the American past, then a new generation of historians stresses behavior and social relations because they see the concept of society as the key to understanding the structure of American history. The synthesis of all of United States history according to the social interpretation is very near, I believe, and so Henretta's article offers a rationale for an approach already predominant in the profession rather than a plea for one seeking to establish itself.[1]

[1] For comparison, see Lawrence Veysey, "The 'New' Social History in the context of American Historical Writing," *Reviews in American History*, 7 (1979): 1–12. On the significance of the culture concept in consensus

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The article not only provides a rationale for the social interpretation of history in general, but Professor Henretta argues for a particular variety of it, one that is extraordinarily influential at the moment in terms of publication and frequency of citation. His solutions to contemporary historiographical problems point to the hegemony of a special brand of social history that has emerged as a major approach to the American past as a result of the demography of the profession as well as of changing intellectual and social currents. The largest age cohort in the profession today grew to intellectual and professional maturity during the foreign and domestic controversies of the 1960s. For many of these scholars the social interpretation of history represents, therefore, a repudiation of the moral and ideological presuppositions of consensus history as well as its conceptual bases.

From this perspective, the social interpretation of American history might be viewed as a revival of the social concerns expressed so often in the Progressive, or economic, interpretation of history that preceded consensus history. If the earlier, Progressive historians emphasized the economic system as the clue to class conflict and social democracy, then many social historians utilize the social system as the basis for understanding the same topics. Thus, the newer interpretation presumes the history of American society as a whole in the same way that the economic interpretation presupposed the history of the overall economy, and both for the same critical as well as analytical reasons. In the newer interpretation the economic system has been placed in the context of the larger social system, but for many of these interpreters the priority of material existence lies at the heart of social criticism and explanation.

THE SUCCESS OF PROFESSOR HENRETTA'S SPECIFIC PRESCRIPTIONS for social history depends upon how well his scheme transcends certain categories of understanding usually considered mutually exclusive or at least difficult of reconciliation. What philosopher Louis O. Mink sees as mutually incompatible modes of comprehension, Professor Henretta argues are fusible in both analysis and exposition. While many philosophers and social analysts argue over the priority of subjectivist and objectivist approaches to explaining human behavior, Henretta proposes a union of phenomenological and objective structuralist perspectives as the resolution. This framework and paradigmatic episodes, he suggests, solve the problems of analyzing change in time, narrative viewpoint, social explanation, and even moral perspective. His essay quite rightly rejects the old problem of Cartesian versus Baconian views on fact and framework and the older arguments over relativism and perspectivism, but, in doing so, he makes certain assumptions about the nature of history and the nature of the historian's task. These assumptions and commitments may be suggested by distinguishing the various kinds of social history Henretta discusses.

Social history definitely no longer means history with the politics left out. On

history, see Robert F. Berkhofer, Jr., "Clio and the Culture Concept: Some Impressions of a Changing Relationship in American Historiography," in L. Schneider and C. Bonjean, eds., *The Idea of Culture in the Social Sciences* (Cambridge, 1973), 77–100.

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the other hand, social history as distinguished from political, diplomatic, and even economic and intellectual history usually deals with fields not covered or only secondarily treated by these other specialties: the family, minorities and migrations, working-class life, social welfare, demography, social mobility, and urban history. In turn, social history in this sense deals with the other specialties as subordinate to its interests.

The *social interpretation of history* places ideas, events, and behavior as well as institutions in the larger context of the overall social system. Politics, economics, diplomacy, and intellectual trends are all explained and connected through their nexuses in specific social systems and preferably in relation to the workings of the overall society. Explanation of electoral behavior in the new political history is a conspicuous example of the trend, but tying professional ideals to professionalization movements is evidence of the same trend in intellectual history. Some intellectual historians even feel beleaguered as a result of this new interpretive turn of events.[2]

The social interpretation of history presumes the *history of society*. Just as the economic interpretation supposedly rested upon a complete analysis of the entire economy, so the social interpretation draws conclusions about the parts of society from its overall nature. Unfortunately, in American history at least, we know too little about the precise nature of the overall social system at any one time, let alone its changing nature over time.[3] Thus, most of the recently hailed monographs basic to the new interpretation more often presume than prove the overall nature of American society upon which they supposedly rest. The best example of this tendency is the many community studies said to be the chief contribution of the new social history. Their findings generally rest upon a sample for which the authors have no idea of the exact universe. Therefore, the cosmos said to be found in the microcosm of the community may be entirely erroneous. Recent efforts at evolving a comparative framework for communities may help place these hallmark studies in their proper perspective—I almost said place.[4] Likewise, inferences about the class system, the corporate structure, and the bureaucratic state all rest more upon intuition than research and certain knowledge. It is always easier to presume the context than to prove it. Just as the consensus historians often presumed the culture that they used to prove their case, so now the new social historians frequently presume the overall society that supports their contentions.

Whether social being in the end determines or merely constrains and conditions human consciousness divides the *social-materialist interpretation of history* in particular from the social interpretation of history in general. All social historians tend to infer ideas from social behavior, but the distinction between the two kinds of social historians hinges upon the exact role human intention and

[2] See the expression of this concern, for instance, by many of the authors in John Higham and Paul Conkin, eds., *New Directions in American Intellectual History* (Baltimore, 1979).

[3] For this same point, see Veysey, "The 'New' Social History in the Context of American Historical Writing," 5.

[4] What such an approach involves is exemplified by two recent studies; see Edward M. Cook, Jr., *The Fathers of the Towns: Leadership and Community Structure in Eighteenth-Century New England* (Baltimore, 1976); and J. Rodgers Hollingsworth and Ellen Jane Hollingsworth, *Dimensions in Urban History: Historical and Social Science Perspectives in Middle-Size American Cities* (Madison, Wisc., 1979).

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ideation plays in "praxis," "habitus," "action," or whatever the intersection between the objective structures and subjective experiences is called. What, in short, is the role of consciousness in the production and reproduction of society that is said to be basic to the social interpretation of history? This question does not ask whether consciousness plays a role in the base as well as the superstructure, to use those time-honored phrases for a moment, but whether the basic direction of influence among the hierarchy of levels is always the same. Should one even think of the levels as hierarchical? Both Raymond A. Williams and Pierre Bourdieu have wrestled with this problem so central to the humanistic, more voluntaristic Marxism of today. Eric J. Hobsbawm has argued that a Marxist in the end must believe in a consistent hierarchy of levels. For history to have direction the basic flow of influences must proceed from material forces of production to institutions and ideas.[5] It is this assumption that separates the social-materialist interpretation from the more general social interpretation of history. It is also this assumption of the primacy of production in everyday life that accounts for much of present-day interest in *mentalité*, in material culture, and in the ethnohistory of ordinary people.

Lastly, must the social interpretation of history, whether materialist or not, be critical of the existing power relations in a society, especially in a liberal capitalist one like the United States? Must the social historian reveal who dominated whom in order to condemn past social arrangements of that society and to construct those of the future? Eugene D. Genovese and Elizabeth Fox-Genovese, in an article aptly entitled "The Political Crisis of Social History: A Marxian Perspective," have distinguished radical from liberal and conservative interpreters of the past according to their willingness to focus upon class conflict and political power in criticism of current social arrangements.[6] Because liberal social historians fail to confront, even if they describe, class exploitation and cultural hegemony, the Genoveses have lumped them with conservative historians. Should we not call the version of social history they advocate *critical social history* or the *critical-social interpretation of history* to distinguish it from a liberal, even if materialist, version?

Only by embracing the varieties of social history listed above can Professor Henretta claim to resolve all the knotty historiographical problems of the relation between historical actors and modern observers, between subjective experience and objective structures, and between critical and analytical history. A phenomenological perspective does not in itself solve the problem of aggregating world views, although it enhances the idea of human agency as central to social change and continuity. If, however, world views are determined as well as constrained during interaction with underlying social and productive forces, then subjective experiences can be inferred from behavior as interpreted by the observer as opposed to the documented thoughts of *all* of the actors.[7] At the same

[5] Hobsbawm, "Karl Marx's Contribution to Historiography," in R. Blackburn, ed., *Ideology in Social Science: Readings in Critical Social Theory* (New York, 1972), 279.

[6] Fox-Genovese and Genovese, "The Political Crisis of Social History: A Marxian Perspective," *Journal of Social History*, 10 (1976): 205–20.

[7] James A. Henretta has taken this premise as his own "epistemological assumption"; see his "Families and Farms: *Mentalité* in Pre-Industrial America," *William and Mary Quarterly*, 3d ser., 25 (1978): 20–21. Henretta is the first historian of the American past to use *mentalité* in the title of an article as far as I know.

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time what Mink has seen as mutually incompatible modes of comprehension in analysis and exposition are readily reconciled by Henretta through his fusion of a particular theoretical framework and a specific kind of narrative viewpoint and moral outlook. Without these assumptions, the historiographical problems he poses remain unsolved.

PROBLEMS OF UNDERSTANDING SOCIAL CHANGE AND CONTINUITY over a long time and of narrative form even remain under Professor Henretta's assumptions. For some interpreters of materialist persuasion, both problems are resolved by portraying the plot of the story as a plot in the story. Emplotment of history is the tale of a plot in history. Henretta eschews such an easy but simplistic form for the paradigmatic episode. But the paradigmatic episode avoids the hardest problem of time by freezing time in much the same manner as the *durées* of the *Annalistes*. What the community study does to space, the paradigmatic episode does to time. Instead of relating all understanding to a single point in space, the episode makes all connections to a single point in time. Dynamic as such a story may be, it fails to treat many such major points over time—many paradigmatic episodes in succession—the quintessential *histoire problèmatique*. Just as the *Annalistes* abandoned the interconnections among *durées* partly because causal connections are more difficult to assert today, so too in its own way the paradigmatic episode avoids the ultimate complexity of causal relationships across long periods of time in favor of the easier (but no less interesting or demanding) task of establishing connections through the focus of a single analytical moment.

Such a focus and narrative mode also sidesteps the problem of expository viewpoint and form demanded by modern consciousness. Whether the social interpretation is critical or not, materialist or not, the problem of narrative form and viewpoint arises in light of the fragmented focus and continuity employed by some of the best twentieth-century novelists to capture the essence of modern experience. Should the new social history follow suit and abandon the omniscient observer's viewpoint for a pluralistic perspective on cause, time, and form? Louis O. Mink has argued that the basic assumptions of old-fashioned, universal history still shape implicitly the modern historian's consciousness, and chief among these, it seems to me, must be reckoned the retention of the omniscient viewpoint in the story.[8] The fragmented perspective and narrative sequence in some *Annales* history is an early effort to bring narrative form into line with the twentieth-century consciousness. Does the action framework and paradigmatic episode advocated in Professor Henretta's essay take a radical stand on this issue, or does it still espouse nineteenth-century narrative veiwpoint and expository form?

ROBERT F. BERKHOFER, JR.
University of Michigan

[8] Mink, "Narrative Form as a Cognitive Instrument," in R. Canary and H. Kozicki, eds., *The Writing of History: Literary Form and Historical Understanding* (Madison, Wisc., 1978), 135–41.

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Reply:

THE COMMENTS OFFERED by Darrett B. Rutman and Robert F. Berkhofer, Jr., raise three issues. The first concerns the historian's approach to the discipline: the conceptual *paradigm*. The second problem relates to the *philosophical stance* or ideology of the author—his or her comprehension of the social world and of human nature. Finally, there is the question of *method*, the techniques of research and rhetoric that constitute the craft of the historian. In each instance, I find myself more in agreement with the arguments advanced by Berkhofer than the views expressed by Rutman.

IS SOCIAL HISTORY only "history with the politics left out," as Professor Rutman suggests? I do not think so and find considerable support for my position in Professor Berkhofer's astute analysis of the paradigm implicit in the work of the "new" social historians. As Berkhofer indicates, many younger scholars repudiate "consensus" history because of their dissatisfaction with its poorly formulated idealist epistemology. Instead, they assume the "priority of material existence" and utilize a conceptual framework that relates "politics, economics, diplomacy, and intellectual trends" to "nexuses in specific social systems." The resulting *social interpretation of history* does not omit politics but assigns political activity a subordinate position. Historians writing from this perspective assume that political decisions affect social existence, but they emphasize the degree to which the social system shapes the character of political institutions. In this formulation society and politics remain inextricably intertwined; good social history must also be good political history.

Yet the primacy given in the *social interpretation of history* to the "overall social system" represents a new conceptual paradigm and, as Professor Berkhofer discerns, differentiates it from both traditional political history and the "consensus" and Progressive approaches to the American past. This "historiographic revolution" (assuming it has or will occur) eliminates neither the "cant" and poor scholarship that Professor Rutman attacks nor the "anarchy" that he celebrates; these continue to exist, but *within* the intellectual framework of the new social approach. In scholarship as in society, revolutions rarely stop the flow of life or the flux of existence, but they often transform their meaning.

This sweeping generalization as to the nature of revolutionary change raises the second issue, the historian's comprehension of the world. Professor Rutman avers that most research "is done in the absence of any philosophical consideration at all." I must disagree. As my article indicates, all historians take philosophical positions—implicitly if not explicitly—with respect to the

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ontological status of the evidence, the relationship between social reality and individual consciousness, and the potential of human nature. Simply to affirm a "commitment to empiricism" does not eliminate these philosophical choices for Rutman or for any other historian. The "facts" have to be explained not in terms of grand metaphysical schemes but in intellectually precise and coherent formulations. Commitment to one or another of the philosophical positions suggested by Professor Berkhofer (*social-materialist*, *critical*, *liberal*, and so forth) cannot be avoided by the historian in his or her daily professional life, but it can be ignored—'s Rutman himself seems determined to do. My own view is that the philosophically self-conscious author or teacher offers a more accurate and more informed interpretation of history.

What stance should a historian take? And what positions are inherent to the *social interpretation of history*? I cannot agree with all of Professor Berkhofer's observations. Many of the new social historians do not embrace "materialism"—as the intense interest in the scholarship of symbolic anthropologists such as Clifford Geertz clearly indicates. Community studies are not flawed by a presumption of the universality of their substantive findings, for their chief contribution has been the explication of social process. There is an important difference between the historian's professional responsibility to comment critically on the power relations and cultural hegemony in past societies and the citizen's obligation in the contemporary world, but Berkhofer confuses the two. Finally, I cannot fulfill his request to "embrace" or to reconcile all of the varieties of social history, for some are mutually exclusive. My article explains why some philosophical positions and analytic strategies satisfy me more than others. I ask only that we as historians acknowledge the existence of these options and, like Pierre Bourdieu and Raymond Williams, "wrestle" with their implications. In so doing, we contribute not only to the more coherent comprehension of the past but also to the intellectual history of our own time.

This brings us, finally, to the question of method—as Professor Berkhofer puts it: "the problem of expository viewpoint and form demanded by modern consciousness." Should historians emulate those twentieth-century novelists who abandon the "omniscient observer's viewpoint for a pluralistic perspective" and a "fragmented focus"? This appealing suggestion is compatible with the phenomenological approach and the action model advocated in my essay. The "total" history of an event can easily take the form of a montage composed of individual life-histories. There is no inherent contradiction between the modern literary consciousness and the phenomenological method; indeed, they are closely related in origin and in technique.

The paradigmatic episode raises more serious methodological issues, as Professor Berkhofer perceptively observes. These episodes are chosen and controlled by the historian and, therefore, embody some of the conceptual assumptions of "old fashioned universal history" with its omniscient narrator. Moreover, this approach neglects important problems of focus and comprehension. It "freezes" time (rather than space, as in the community study, or

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pattern as in the *durées* of the *Annalistes*), thereby avoiding "the ultimate complexity of causal relationships across long periods of time." These points are well taken and suggest some of the very real limitations of *histoire problème*. Nevertheless, this method succeeds in many instances and resolves more technical and interpretive problems than the three-tiered program of research advocated by Professor Rutman. Should we assign Ph.D. candidates the task of digging out the facts on mortality in the early Chesapeake, then pass on the data to tenure-conscious assistant professors for cautious articles connecting life expectancy and family structure, while reserving to the grand masters of the profession the awesome task of relating these developments to the American Revolution? Is this "cumulation and cooperation" or elitism? In my view the historian should work simultaneously on all three of Rutman's levels, uncovering new empirical data (which can then be used by other investigators) and using it to address an important problem of social process or historical interpretation. Will this scholarship be "idiosyncratic and subjective" or prey to easy generalization? Perhaps, but a naive empiricism poses greater dangers because it diminishes the historian's responsibility to make a considered judgment not just on the validity of the "facts" but on their social meaning and moral significance. "Values," E. P. Thompson has argued, "are not 'imponderables' which the historian may safely dismiss with the reflection that, since they are not amenable to measurement, anyone's opinion is as good as anyone else's. They are, on the contrary, those questions of human satisfaction, and of the direction of social change, which the historian ought to ponder if history is to claim a position among the significant humanities."[1]

MOST DISCUSSIONS OF INTELLECTUAL PARADIGMS, philosophical or ideological positions, and historical methods reflect the experience or the aspiration of their authors. They may transcend their origin by articulating widely held but inchoate assumptions or by stimulating fresh thought and vigorous debate. I feel challenged by Darrett Rutman's comments and enlightened by those of Robert Berkhofer; I thank them both.

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[1] Thompson, *The Making of the English Working Class* (New York, 1966), 444.

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Reviews of Books

GENERAL

EDWARD W. SAID. *Orientalism.* New York: Pantheon Books. 1978. Pp. xi, 368. $15.00.

Orientalism, the body of opinions concerning the Islamic Near East held by Euro-American scholars, imaginative writers, and officials, presents itself to Edward W. Said as a major feature of post-Enlightenment Western culture that requires analysis by employing Michel Foucault's notion of a discourse. Orientalism, according to Said, had its roots in the older Christian relation to Islam but was modified by the secularizing currents of the Enlightenment and took its definitive form in the late eighteenth century and the first three-quarters of the nineteenth. Orientalism was structured by Western science—natural and humanistic—with its comparison and classification and consequent subjugation of the particular to the universal, the individual to the stereotype. Thus Islam and the Orient became eternal, unchanging. Orientalism, arising in an age dominated by a European sense of superiority, consigned the Oriental to the inferior, the backward, even the degraded, and easily gave scientific status to theories of racial superiority and to justification of colonial rule. Orientalists, moreover, by discovery, decipherment, and selection recreated the Orient and thus became possessed of a sense of power: the Orientalist, not the Oriental, was the actor. The new science quickly achieved authoritative status, and subsequent practitioners could work only within the limits of the discipline. Orientalism today has changed in no essential way. Racism has been abandoned, but the eternal, premodern Orient (or Islam) remains, in need of tutelage. The only significant addition is the Orientalist as mobilizer of the Orient for "our" side, with representatives ranging from T. E. Lawrence to the "area specialist."

This reviewer has no quarrel with Said's exposition of Orientalism's essentials, though Orientalism is not quite so homogeneous as he believes. His analysis of Orientalism's relation to the main currents of Western thought is persuasive and significant, even though organization and style obscure the argument. Said's presentation conveys the impression that Orientalist scholarship has been radically different from the other humanistic disciplines and even of low quality. Read very closely, however, Said is of two minds. He indicates an awareness of the difficulties intrinsic to generalization, he depicts the founders of Orientalism as typical of nineteenth-century learning, and he acknowledges that they, and some of their successors, made genuine contributions to knowledge. But these remarks are rare and brief. In the discussion of contemporary Anglo-American Orientalism, his ambivalence disappears. Orientalism alone among the humanistic disciplines occupies itself with trivia. Orientalists alone approach their subjects motivated by hostility, not sympathy. This reviewer refrains from comment on either point.

Said's intention, apparently, is to show how Orientalism came into being as the doctrine and the corporate institution for exercising Western domination of the Orient (see especially p. 3). Unfortunately, apart from the demonstration of the relationship of Orientalist beliefs to European science and scholarship, reiterated vague references to "imperialism" constitute the totality of historical analysis. As he continues his projected studies, it is to be hoped that he will give consideration to what, besides the Orient, the Orientalists had in mind. He may discover that Europeans and Americans, when speaking about or acting in the Orient, have been quite as much concerned with other Euro-Americans as with the Orient, just as Near Easterners, when speaking about the gap between East and West, have been primarily concerned with other Near Easterners.

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BRONISLAW BACZKO. *Lumières de l'utopie.* (Critique de la Politique.) Paris: Payot. 1978. Pp. 416. 99 fr.

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Was it coincidence that an edition of More's *Utopia* appeared in France in 1789? The author of *Lumières de l'utopie* thinks not. The desire to shape a new order of things in the light of a utopian vision—an imaginative representation of a society defined by its systematic alterity to that which exists—lies at the heart of Bronislaw Baczko's study of the "social imagination" of late eighteenth-century France. Like Michelet, he is concerned with the French Revolution as a cultural and political act: the collective act of transforming the moral grounding of an entire community. In the course of such an act, utopia enters into history—history experienced not as continuity but as rupture—and everyday life is reconstituted in the light of new possibilities and meanings. Utopia ceases for that moment to be "utopian"—ceases, that is, to be "imaginary" rather than "real"—and presents itself as reform, as science, as logic of the past or promise of the future. There is an interpenetration between society as dreamed and society as lived.

In a fascinating work that defies brief review, Baczko explores this interpenetration in three principal domains: history, festival, and urban architecture. In each case, he seeks to move from a consideration of that domain in utopia (the way in which it is treated within the utopian literature of the Enlightenment) to an analysis of the utopian in that same domain of revolutionary action and consciousness. No simple theme emerges—this is not a linear story of the development of particular ideas. Instead, the analysis discloses a series of interrelationships—structural analogies, tensions, and transformations—defined by the dialectic between ideal imperatives and conditions of possibility. Thus we are led from the antihistory of imaginary voyages (in which "history" appears as "story," the transparent account of rational human actions quite the contrary of the obscurities and irrationalities of history as experienced in the *ancien régime*) to the fusion of history and utopia in Condorcet's idea of progress (in which the ideal society is conceived as a realization of history, and utopian speculation takes the form of scientific prediction). Simultaneously, we are reminded of the deep sense of rupture implied in Condorcet's determination to reclassify the data of his historical science by dating events backwards and forwards from the advent of the French Republic in 1792, a sense of rupture that found fullest expression in the reconstitution of social time by means of the revolutionary calendar. This revolutionary reconstitution of social time found its analog in a complementary reconstitution of social space, not only in the decimal (or "republican") system of weights and measures but also in the plans, projects, and dreams of a new urban architecture—a "revolutionary" architecture that, as conceived by architects such as Boullée and Ledoux, would spatialize republican values with the clarity and symbolic force characteristic of the ideal cities of the utopian imagination. Little of this architectural ideal found actual expression in stone, yet it appeared in ghostly form in some of the prints depicting the great revolutionary festivals (superimposed, as it were, on the historical city itself) just as it took ephemeral form in the plaster props of these festivals (statues and temples, altars and *arcs de triomphe*) and the conceptual remapping of the city enacted in their processions. Thus the festival, which frequently appears in utopia as the model of a particular ideal of sociability, emerges in revolutionary France (as in Rousseau's *Poland*, which Baczko provocatively characterizes as "an imaginary voyage") as a means by which a civic ideal is energized and acted out, realizing itself as imaginative recreation suffuses and transforms the world of everyday, historically given, experience.

This is an imaginative and stimulating book, suggestively bringing together the results of scholarly work in areas of historical research that rarely share the same covers. It will be of interest not only to eighteenth-century scholars but also to intellectual, social, and cultural historians generally.

KEITH MICHAEL BAKER
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LEONARD KRIEGER. *Ranke: The Meaning of History*. Chicago: University of Chicago Press. 1977. Pp. xii, 402. $23.00.

In a busy world that tends to summarize a famous man's life work in a cliché, Leopold von Ranke's summary must be the dictate to write history "wie es eigentlich gewesen." Behind the cliché lurks an impression in most historians' minds that Ranke helped found the modern discipline of history by insisting on the careful use of documents to tell the story "as it actually was." His faith in a concrete, individual, and diverse reality, reflected in the documents, to this day sustains much historical investigation in the teeth of criticism by philosophers, science theorists, and social scientists, who have almost given up explaining to historians how unscientific their work is.

But Ranke the cliché was not Ranke the man. Who Ranke was is still a partial mystery, since no full and adequate biography exists to this day. Leonard Krieger has set out to illuminate this mystery by providing an intellectual biography of Ranke. Although based almost entirely on printed sources (Ranke's papers are not very helpful), Krieger's exploration of Ranke's developing mind is quite exhaustive. From Krieger's point of view, Ranke's insistence on history as it actually was constituted only one aspect, a sort of resignation and

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subjectively perceived defeat of his yearning for a higher synthesis and what one might be tempted to call "covering laws" of history. The central theme of Krieger's work is Ranke's lifelong conflict between a drive to respect the documents and their descriptions of unique events, on the one hand, and a desire to find behind them general, unifying ideas, on the other. The work of the mature historian on "universal history" was only the attempt to fulfill a youthful dream that soared above the dusty pages of the Venetian *Relazioni* and the Prussian archives.

But Ranke was not, in the end, a positivist, despite his understandable nineteenth-century yearning for general laws in history. Ranke could not soar above his training and origins. His universalism tended to be bounded by his deeply felt Christian view of the world and his conservative politics. He was well over fifty before he began to realize that he lived in a century of revolutions. As Krieger shows in his illuminating but all too brief explorations of Ranke's life, Ranke was inclined to hold at bay all those personal influences that might have provided confrontations so that he could get on with his work. As Krieger also points out, if very gently, Ranke courted rather than criticized the powerful and influential, cared little for academic freedom, and lived, in short, a superficial external life in order to devote himself to what mattered to him. As anyone who has studied his external life, academic performance, or general attitudes knows, Ranke remains an ambivalent and even uninteresting man. Krieger has quite appropriately concentrated on the mind of Ranke. He discovers there a torn and searching attitude toward the meaning of history, rather than a single-minded cliché.

This is strictly an intellectual biography of Ranke the historian. It is often repetitious, wordy, and a little hard to follow. But unlike Ranke, Krieger is rarely confused or opaque. This is a painstaking and admirable attempt to enter the mind of an important historian. Although it will still not stand as the biography of Ranke that nobody has been able to write, it will aid a future biographer immensely and will meanwhile serve as one of the most diligent, balanced, and thorough works of insight into the inner workings of the lifelong thought process that resulted in a canon for subsequent generations of historians.

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G. A. COHEN. *Karl Marx's Theory of History: A Defence.* Princeton: Princeton University Press. 1978. Pp. xv, 369. $18.50.

In a field crowded with books that either paraphrase Marx or attempt to translate him into a more fashionable (and generally more obscure) language, G. A. Cohen attempts to reconstruct historical materialism in a clear and analytically precise fashion and to argue in its favor. Cohen himself is a professional philosopher, and he brings to this project the clarity and argumentative rigor of the analytic tradition in Anglo-American philosophy. His is an ambitious and impressive work, one which should prove of signal importance.

The historical materialism that Cohen champions is an old-fashioned kind, in which history is fundamentally the growth of human productive power, and the different forms of society rise and fall because they first enable, then impede, that growth. Although this seems a straightforward reading of Marx, it is not one contemporary Marxists have been happy to defend. Cohen takes pains to show that it was in fact Marx's view and—what is a distinct issue—that it is a plausible one. In carrying out these tasks Cohen analyzes the basic concepts of historical materialism and explicates such Marxian theses as the historic role of capitalism and the necessity of class rule prior to socialism. Of special interest is Cohen's dissection of the theory of fetishism and his novel development of Marx's distinction between the social and material properties of society.

Marx's theory, in particular the claim that production relations have the character they do because, in virtue of it, they promote the development of the productive forces and that the nature of the superstructure is to be explained by its function of conferring stability on the production relations, seems to involve a commitment to functional explanation. Philosophers, however, have tended to reject functional explanations in principle or to contend that they collapse into causal accounts, while Marxists have been chary of them because of their association with the functionalism of anthropologists like Malinowski and Radcliffe-Brown. Cohen devotes two chapters to elucidating and exonerating the notion of functional explanation, and his challenging discussion is likely to stimulate philosophic debate beyond the world of Marx studies. Similarly, his final chapter, which diagnoses contemporary capitalism as irrationally biased in favor of expanding output as against reducing toil, is of independent significance.

Cohen writes with limpidity, verve, and honesty. He puts his arguments up front, is candid about their limitations, and points out routes available to critics. Specialists may take issue with him on points of exegesis, but the important question is the scientific viability of the theory Cohen constructs on Marx's behalf. Some friends of Marx may feel that in putting forth such a bold and precise version of historical materialism, Cohen has rendered it too susceptible to refutation. But such empirical vulner-

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ability is the hallmark of scientific theories. If in the end, despite Cohen's able advocacy, Marx's theory is found untenable, it will not do to pretend that he had something vaguer in mind.

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BARBARA WOLFE JANCAR. *Women under Communism*. Baltimore: Johns Hopkins University Press. 1978. Pp. x, 291. $16.00.

Barbara Wolfe Jancar explores the status of women in Eastern Europe, the Soviet Union, and China. She defines status as the position of an individual within her society, and the status she would have for women is equal, that is, having access to the same jobs as men and, more important, to the same political power. Judged by this standard, women in Communist nations have not achieved equality, despite their governments' espousal of female emancipation. Jancar sets out to discover why.

She finds an answer that is well known: women still invest much of their time and most of their emotional energy in their families. In part they do this out of necessity, since social services adequate to free them from housework and childcare have not been provided. Women have also been encouraged by their governments to see motherhood as a valuable service to society, one to which they should devote themselves. And finally, women in socialist nations have retained traditional values. They choose to spend themselves on their families at the expense of careers because they believe that women are naturally suited to the domestic role. Jancar documents this last assertion with interviews among women in Eastern Europe and among émigrés. She concludes that Communist governments have added work outside the home to women's traditional roles, a real but limited change in women's status with which the women themselves seem content.

The outlines of female emancipation in the Soviet Union are familiar. Jancar adds data from Eastern Europe and China that reveal the same pattern there: social services, protective labor legislation, maternity benefits, mobilization of women for industry, an emphasis on motherhood, and a persistent male-female division of labor in the family. She lays this similarity of experience throughout the Communist world to the common ideology and programs of Communist regimes, and to the processes of industrialization through government fiat. She acknowledges the great strides toward female emancipation taken in Eastern Europe and China but argues that the limits of change may now have been reached. Any further alteration in the power relationship between men and women might threaten the status quo and, with it, the governments. Pluralistic societies, although slow to liberate women in the early years of industrialization, are now showing a greater capacity to open new opportunities to women than are the increasingly conservative Eastern European governments.

This is a plausible, coherent, eminently liberal argument, one often advanced by American students of the Communist approach to women's liberation. It measures the achievements of Communist governments by a Western, feminist standard, finds them wanting, and asserts that the governments could have done more. The argument usually ignores the fact that the women and men of these nations show no interest in their governments' doing more. Jancar documents women's attitudes toward the family and female personality, but even she is loath to admit that the androgynous equality which contemporary feminists seek, and by which goal the Eastern Europeans are measured, has never been accepted as desirable either by Communist parties or by their followers. Throughout the Communist world, governor and governed alike cling to the nuclear family and to woman's traditional role within it. Woman is seen as its center, not only its chief worker, but its heart, and it has become a refuge in a rapidly changing world. Rather than accuse Communist leaders of conservatism, Jancar should ask why the nuclear family, a blend of the traditional and modern, has proved so strong in so many cultures. That question answered, she would be well on her way to a sophisticated understanding of the reasons why women have not achieved equal status with men in Communist societies, or anywhere else.

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VERN L. BULLOUGH and BONNIE BULLOUGH. *The Care of the Sick: The Emergence of Modern Nursing*. New York: Prodist. 1978. Pp. 311. $15.00.

The Care of the Sick is a history of nursing from primitive times to the present. Almost a third of the book is devoted to the period prior to the mid-nineteenth century when nursing began to emerge as a respectable occupation for women. Unfortunately, the amorphous nature of early nursing combined with the limited information available does not lend itself to such an extensive treatment. The result is that this first section is essentially a history of Western civilization with an emphasis upon nursing and medicine. As Vern L. Bullough and Bonnie Bullough move into the modern period, however, the history comes into its own and provides an excellent survey of the rise of the nursing profession.

Florence Nightingale, who symbolizes the profes-

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sion, undoubtedly made nursing respectable in the English-speaking world and rightfully deserves credit for this and the other reforms she espoused. At the same time, as the authors point out, in her efforts to appease opposition to her program she leaned over backwards to make her nurses subservient to the dominant male physicians; she was a product of a caste system that she incorporated into her nursing structure; and she was more concerned with the character of her protégées than their ability as nurses. The Nightingale nurse-training system was transferred to the United States largely intact, although one major change was made in the administrative structure. Whereas the matron who directs English nursing schools is solely responsible to the nursing school board, American nursing schools were placed under the direction of the hospital board of trustees. Since hospital directors are often male physicians, the position of the American nursing superintendent is not nearly so strong as that of her English counterpart.

The proliferation of hospitals at the turn of the nineteenth century was accompanied by a comparable growth in hospital nursing schools, most of which gave minimal instruction and used nursing students to provide hospitals with a cheap form of labor. The result was an overabundance of graduate nurses competing for the limited private practice. By fighting successfully to raise education standards, nurse organizations forced these schools to close, thus requiring the hospitals to employ graduate nurses. In addition, the entrance of the government into medicine on a large scale beginning with World War II created many new nursing opportunities, thereby contributing to improving the low pay and deplorable working conditions of many nurses.

The struggle by nurses to achieve professional status was even more difficult than that for the other occupations open to women—teachers, secretaries, and librarians. The latter fields were not competitive with males, and women who engaged in them could always count upon male allies. Nurses, however, represented a threat to the medical profession, one dominated by males and one that was desperately seeking to improve its own economic and social status. Even today, despite considerable improvement, nurses have little voice in hospital administration nor in deciding what constitutes good nursing service. Only one of the twenty members of the Joint Commission on Accreditation of Hospitals is a nurse, and nurses do not have full voting rights on the National Professional Standards Review Council.

Although *The Care of the Sick* covers nursing on a worldwide basis, its major emphasis during the past one-hundred years is upon American developments, and the authors' comments upon nursing today are perceptive. Their work will probably be the standard history in English for some time to come.

JOHN DUFFY
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RODGER SWEARINGEN. *The Soviet Union and Postwar Japan: Escalating Challenge and Response*. Foreword by EDWIN O. REISCHAUER. (Hoover Institution Publication, number 197.) Stanford: Hoover Institution Press. 1978. Pp. xxv, 340. $14.95.

This book's purpose is to add to our comprehension of the critical relationships that exist between the Soviet Union and postwar Japan by providing a survey of ideological, political, diplomatic, and economic problems that occur between these two nations. It consists of five parts: a historical background, a discussion of Soviet policies and practices vis-à-vis occupied Japan, an account of Russian intentions and techniques in the period of normalization after the Occupation, a survey of the economic relations between these two powers, and, finally, an analysis of diplomatic and strategic issues, such as the fisheries question, the northern territories problem, and a general survey of the strategic dimensions that exist in the new northeast Asian triangle formed by the USSR, the PRC, and modern Japan.

To the question of whether the author fulfills his design, the answer would have to be an unqualified yes, but to the question of how well he does this, the reviewer is forced to be much more hesitant and cautious in his response by answering that this is an uneven book in terms of lack of analysis, sophistication of perception, and depth of research.

To start on the negative side of the ledger, there exist a number of factual blunders, especially in the first chapter that deals with history, possibly reflecting a cavalier attitude toward Clio taken by political scientists. Page one begins a survey of Russo-Japanese relations by stating that "the Russia of Catherine the Great was not particularly concerned with Japan," but no one doubts that the Russia of Peter the Great was (what about Denbei, the Spanberg and the Bering voyages?). On page five there is not only the statement that Russia initiated the Triplice (Germany did) but also that Japan controlled a vital section of the Chinese Eastern Railway (the author confused it with the South Manchurian railway). Later on one is also told that the united front strategy was devised to "placate Japan" and that Yalta saw "an exorbitant price paid to the Soviet Union" (p. 16). This leads to another criticism. There is a certain naiveté and Cold War rhetoric that pervades statements pertaining to the Soviet Union, whether the author describes the

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Ukraine as a "remote and backward region" (p. 18), speaks of "simple Marxist-Leninist logic" (p. 38) (assuming that there is some agreement on what that might be), or treats the Japanese Communist Party as a simple tool of Moscow. Much of the description of the Soviet presence in occupied Japan and of Russian techniques used to indoctrinate Japanese prisoners-of-war is taken exclusively from American intelligence sources, and while they may reflect quite accurately Soviet intentions, the absence of Russian sources and the Soviet point of view creates doubts about conclusions. The level of analysis does not rise perceptibly in the chapter dealing with the checkered career of the Japanese Communist Party.

On the other hand, I found the later chapters of the book dealing with more concrete and less ideological issues far more satisfactory. The discussion of joint economic ventures in Siberia and the description of trade relations between Japan and the Soviet Union are handled in an interesting and clear manner. An especially useful and well-presented section of the book is devoted to the two perhaps most difficult problems, that of the fisheries and of the northern territories question. The latter, being the most central, most difficult, and most persistent problem between the two nations, is also one on which the USSR has taken an intransigent and diamond-hard position.

In conclusion, I feel that this is a very useful and informative book, containing much interesting material, but also one that could have been much better if it had been written with somewhat less obvious prejudice and greater analytical depth.

The appendixes contain texts of all treaties and agreements between Japan and the Soviet Union. There is no bibliography.

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ANCIENT

W. K. C. GUTHRIE. *A History of Greek Philosophy.* Volume 5, *The Later Plato and the Academy.* New York: Cambridge University Press. 1978. Pp. xvi, 539. $42.50.

This volume continues W. K. C. Guthrie's erudite, comprehensive, and entirely readable history of Greek philosophy. The present work brings to completion the study of Plato's thought that commenced with its immediate predecessor, volume four. The focus is on the later development of Plato's doctrines—the later dialogues—with due attention to scholarly problems of date and the probable order of composition of the individual dialogues. In addition to a survey of dramatic elements and analyses of the philosophic content of each of the dialogues, valuable information is supplied concerning characters, historical allusions, and context (in so far as these matters are known or reliably inferred). On issues of special importance and interest, Guthrie provides excursuses, supplementary notes, and appendixes bearing on his discussion of the main text. These reflections and reconsiderations are illuminating and often highly informative; they are skillfully related and adjusted to the central discussion of the dialogue in question so as to enrich our understanding and appreciation of—rather than deflect attention from—the dramatic and conceptual movement of ideas.

The dialogues here treated in successive chapters are: *Cratylus*, *Parmenides*, *Theaetetus*, *Sophist*, *Politicus* (*Statesman*), *Philebus*, *Timaeus* and *Critias*, and the *Laws*. Further chapters deal with the doubtful and spurious dialogues, the Letters, and with Plato's alleged "unwritten" metaphysical doctrines and teachings. There is also an extended survey of eighty-six pages on Plato's associates and successors in the Academy: notably Eudoxus, Speusippus, Xenocrates, and Heraclides Ponticus.

Space does not permit even a cursory report of the multitude of topics and turns of thought that occur in the course of these sympathetic and sensitive expositions of Plato's writings. The discussions are throughout carefully and judiciously conducted; controversial points of interpretation, research, and recurrent dispute are fairly acknowledged; and helpful bibliographical references to both general and specialized works on the subjects under consideration are offered in the text and in footnotes. A valuable bibliography, index of passages quoted or referred to, and a thorough general index are also supplied.

In all, Guthrie has managed with remarkable success to organize and draw on a vast body of often diversified and recondite source materials and commentary—ancient and modern, scholarly and philosophic—all of which is necessary and relevant to a scrupulous and balanced history of Plato's philosophy, the meanings to be found in it, and its lasting pertinence.

Nothing human is flawless, as Plato would be the first to point out. But it is exceedingly difficult to find any substantial fault in Guthrie's volume and equally difficult to resist praising it. This book is a major contribution to historical and philosophical scholarship and one from which the layman and specialist alike cannot fail to profit.

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J. K. DAVIES. *Democracy and Classical Greece.* (Fontana History of the Ancient World.) London: Fontana. 1978. Pp. 284. $5.50.

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This book does not provide what the title seems to promise, what the newly appointed Professor of Ancient History at Liverpool is so well qualified to supply, and what is still so badly needed in classical studies, an analysis of democracy in the social, economic, intellectual, and political environment that gave it birth and yet so often sought to strifle or destroy it. The "Democracy" of the title is not the subject or unifying theme, but one of many motifs whose recurrence is emphasized in this treatment of Greek history from 478 through 336 B.C. The work is in fact the first of a series of volumes covering ancient Greece for the Fontana History of the Ancient World. The series aims at presenting an "up-to-date account," citing "the more important evidence," integrating selected documents into the narrative, and discussing the principal arguments concerning them. All this while covering 140 brilliant and complex years in less than 300 pages! That the result is disproportioned, uneven, and often frustrating is not surprising; what is welcome is the frequency with which the book raises new questions, sheds light on old problems, and provokes fresh understanding.

J. K. Davies wastes no time struggling to retrieve many of the old chestnuts of Greek history from the dull glow of traditional scholarship. He abandons them—the ever virulent Great Man theory of history, the familiar Xenophontic style of Greek historical narratives, indeed even continuous narrative itself—for a more analytical account that emphasizes recurring themes and that attempts "to pick out such seemingly accurate detail as we can and to allow patterns to emerge," even if the documentary foundation for the conclusions is sometimes "shaky" (p. 142). The patterns that emerge are often social and economic themes, which here receive a welcome emphasis, and sometimes geopolitical patterns, as in the stimulating chapter on Athenian imperialism.

But it is the last chapter, perhaps, that reveals most clearly the strengths and weaknesses of Davies's approach. Here Davies groups together under the title "The Opportunists" a very diverse group of fourth-century leaders, whose power he contends was based on mercenary troops and who managed to establish a dynasty. Dionysius I of Syracuse is the best example of this pattern; Jason of Pherae, Mausollus of Caria, Clearchus of Heraclea are other instances. The similarities are impressive and revealing. Then Philip II of Macedon is fitted—forced—into the same mold. His power is explained as the result of his ability to "add a second role as mercenary commander to that of the traditional king" (p. 249). He is able thereby "to move opportunistically into the gaps left by the interaction, spoiling, and internal contradictions of the major powers" (p. 228). This approach underestimates the significance of Philip's development of a truly national army and the deeper structural changes that took place in Macedon during his reign. It needs reformulation, qualification, and a depth of presentation impossible within the limits of this book. To some it will seem a particularly damaging example of Davies's preference for the first of the alternatives he sees in the study of Greek history: "as so often . . . the choice is between the risky creation of an interpretation and the stultifying regurgitation of what the surviving sources happen to say" (p. 95). But whatever its limits, whatever its excesses, Davies's book provides a fresh approach and some ideas that both layman and professional will find worthwhile to ponder. No small accomplishment!

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CHARLES D. HAMILTON. *Sparta's Bitter Victories: Politics and Diplomacy in the Corinthian War.* Ithaca: Cornell University Press. 1979. Pp. 346. $17.50.

Charles D. Hamilton's theme is the relationship between internal politics and foreign policy in Sparta, Thebes, Athens, and Corinth, between 405 and 386 B.C. In Sparta, he detects three factions: (1) King Pasanias's: anti-Lysander, anti-imperialist; (2) King Agis's: standing for strong foreign policy; (3) Lysander's: brutal, unscrupulous imperialism. Lysander makes his boy-love Agesilaus king; the Spartans attack Elis and support oligarchies in Ionian cities. In Thebes, Ismenias leads the pro-Athenian faction, Leontiades the pro-Spartan. In Athens, Plato (!) and Xenophon represent oligarchy, Thrasybulus moderate democracy, Epicrates and Cephalus radicalism. Persian gold rebuilds the Long Walls and subsidizes Conon's fleet against Sparta. In Corinth, oligarchy, conservative and agricultural, has ruled since the sixth century B.C., but commercial interests join petty tradesmen, artisans, and sailors in a democratic faction that first unites with Argos (isopolity) and then succumbs to it. A border dispute, exploited by Thebes, brings on the eight-year-Corinthian War, sparked by resentment at Spartan selfishness, greed (failure to share spoils of the Peloponnesian War), arrogance, and aggressiveness. Lysander dies in battle at Haliartus (395), the Spartans call Agesilaus home from successful Anatolian campaigns; this breeds in him an obsessive and long-lasting hatred of Thebes. After his dubious victory at Coronea (394), there are no more pitched battles, but resources and morale are heavily drained. Conon's naval victory off Cnidus (394) is fatal to Spartan imperialism. Anatolian Greek cities ally and strike a common coinage. Conon dies (392), but under the innovative peltast-commander

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Iphicrates Athens aims again at empire. City after city, island after island come over to Athens, in disgust at Spartan misrule; Athens regains her grain-route from the Black Sea. But the clever Spartan diplomat Antalcidas makes a peace that betrays Ionian cities to Persia, while declaring mainland ones autonomous, thus breaking up the Boeotian Confederacy and the Argive-Corinthian isopolity. Corinth restores oligarchy; Athens will soon form its Second Confederacy. Sparta wins, but its victory is hollow: it is hated as the betrayer of Ionian cities. The Peace of Antalcidas perpetuates a disunity that will facilitate Philip of Macedon's victory at Chaeronea (338).

Hamilton has been ill-served by his publisher, a university press that has allowed a deplorable style well-nigh to conceal a cogent argument. The book teems with solecisms ("comprised of," "hopefully," "likely" as adverb, "discomfort" for "discomfit," "shall" for "will," "as" for "like," dangling phrases, false references for pronouns, unidiomatic use of prepositions), clichés ("maintain low profile," "grow like a cancer"), mixed metaphors ("trigger a root cause," "speech riddled with improper emphases"), tautology ("fellow equals," "residential seat," "price of risking," "faithful loyalty," "ready in advance," "internal revolution," "looking back in retrospect"), jargon (over 120 examples of words ending in -tion), excessive use of the passive, repetitions (especially in chapter nine, a reprinted article), errors in printing Greek, otiose quotation marks, illogicalities (same man ordinary and prestigious; Iphicrates resigns "willingly . . . in disgust"). The author should emulate Grote as well as cite him; the press should find editors who will set a high standard. Editors should not allow ancient historians, of all people, to contribute thus to the *trahison des clercs*.

PAUL MACKENDRICK
University of Wisconsin, Madison

MICHAEL GRANT. *History of Rome.* New York: Charles Scribner's Sons. 1978. Pp. xviii, 537. $17.50.

The indefatigable Michael Grant has added yet another book to a list that now approaches four score. Like its predecessors, it is readable as well as scholarly and, already offered by one book club, is assured of a mass readership. High volume makes it possible to produce a handsome book from beginning to end. The illustrations, though not in color, are numerous, well chosen, and neatly placed. They include a head of Hadrian found only recently.

This is the sort of volume that Grant does best. Though he is knowledgeable and imaginative, his more scholarly works, as in numismatics, though respectable, have been less well received. Here, Grant's gifts for happy selection of material, organization, and felicitous expression can come into full play. The language can delight the reader: of Nero, Grant writes that the sculptors of the time infused "just that touch of elevation that made the emperor seem, if still a fat lout, at least a lout of superhuman dimensions" (p. 305).

Grant's astonishing output in part results from a considerable overlap in his published works: he has previous surveys of Rome, biographies of numerous important Romans, a history of the Jews, biographies of Herod, Jesus, and Paul, plus books on myth, literature, and archeology. In this history we thus get a more detailed Jewish background than usual and more on early Christianity; two previous volumes on Rome after 161 A.D. probably explain why the latter section of this book is rather full. Nothing in Grant's earlier works, however, explains why he chose to devote fifty uncharacteristically dull and badly organized pages to the uncertain history of the early Republic when half that number would have done nicely. Surprisingly, Roman literature gets rather short shrift: Martial and Juvenal, for example, get only a brief paragraph each.

Invariably, a reviewer finds interpretations he thinks faulty and generalizations that mislead. There are also errors and lamentable omissions—too many—that perhaps reflect the speed with which the author writes and publishes. Grant manages to summarize the early Republican constitution without mentioning the important *comitia tributa* (p. 74). He refers to a tradition that the entire Torah was inscribed on Moses' two tablets (p. 334), and he describes Egypt as rich in iron (p. 134)! His implied definition of "middle class" so as to make it quite large and the "backbone" of the Roman state (p. 447 and elsewhere) can only be described as peculiar.

The apparatus is minimal. The charts of ruling families of the high Empire do not include the dates of the reigns. The bibliography, obviously compiled quickly (the name of the reviewer is misspelled!), seems more representative of modern authors than judiciously selective.

Yet, again, Grant is a popularizer of the very best sort who knows the period and the evidence, and he writes—mostly—superlatively well. This fine volume may be highly recommended for public libraries, undergraduate libraries, the general reader, and perhaps also for lower class survey courses.

HENRY C. BOREN
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C. P. JONES. *The Roman World of Dio Chrysostom.* (Loeb Classical Monographs.) Cambridge: Harvard University Press. 1978. Pp. vi, 208. $15.00.

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Dio Chrysostom ("Golden-Mouthed"), from Prusa in northwestern Asia Minor, was a leading Greek orator in the Roman Empire (born ca. A.D. 40-50, died after 110). Philostratus in his *Lives of the Sophists* accorded him space in a preliminary survey of "philosophers who were thought to be sophists" (C. P. Jones has a good discussion of the several, often contradictory, meanings that attached to the appellations of "sophist," "philosopher," and "rhetor"). A friend of Vespasian and Titus, Dio was exiled by Domitian. Following a series of wanderings, he was recalled after Domitian's assassination (A.D. 96). Under Trajan his reputation was at its height; his close connection with that military emperor is illustrated by Trajan's remark as Dio rode next to him in the golden chariot in which emperors led the procession back from the wars: "What you are saying I do not know, but I love you as myself."

Jones's book is a companion to his earlier *Plutarch and Rome* (1971). Both Plutarch and Dio stood at the threshold of the "Second Sophistic," a movement well sketched by G. W. Bowersock in his *Greek Sophists in the Roman Empire* (1969). All three books, in fact, share a common aim, as their titles proclaim: to fit these Greek writers and movements into the historical context of the Roman Empire as a whole. Most members of the upper class—emperors, consulars, senators, local dynasts—whether born in the East or the West, tended to form a fairly cohesive ruling and intellectual elite, because their educations and literary interests were much alike.

Jones's book consists of fourteen chapters. The first two are introductory ("Prusa," "Education and Youth"), the last ("Rome") a general conclusion. The rest are ordered chronologically according to Dio's extant orations—a sensible schema, since what we know of Dio derives mostly from his own speeches. Because most orations were addressed to particular cities of the Eastern Empire and were delivered on specific occasions, a fascinating picture emerges: the rivalry among cities—and among their highly placed patrons such as Dio; the factional hostilities within these cities, of which Dio's checkered experiences in his native Prusa constitute the most telling example; the relations of these cities with the Roman government, and the attempts by men such as Dio to act as advocates and counselors. The especially interesting seventh chapter analyzes two orations in which Dio depicts in utopian terms the rugged life of the peasant (Euboean) and the simple self-sufficiency of a community on the edge of the "civilized" world (Olbian). Once we have taken into account Dio's special pleading, Jones convincingly argues, there is no reason not to accept as basically true the residuum—a most valuable resource, since so little survives from antiquity that concerns the lower classes (Dio's definition of poverty is worth repeating: "the condition of having to work for a living").

Jones eschews much discussion of Dio's "thought," which is probably just as well. Nevertheless, his views on the complex question of Dio's philosophical allegiances should have been summarized toward the end. All in all, however, this is a useful book on an important author, marked everywhere by sane judgment.

T. J. LUCE
Princeton University

HERBERT WAGNER *Studien zur allgemeinen Rechtslehre des Gaius: Ius Gentium und Ius Naturale in ihrem Verhältnis zum Ius Civile.* Zutphen, Holland: Terra. 1978. Pp. xxiv, 290.

Opinions differ about the significance of Gaius. Herbert Wagner cites two extremes—the Soviet scholar R. J. Wipper, who regards the second-century jurist as "the greatest reformer of Roman law," and Wolfgang Kunkel, who dismisses him as a third- or fourth-rater. No one, however, would deny the significance of the attitudes and ideas preserved in his *Institutes*. For Wagner, he was the jurist who brought rational order and historical perspective (he speaks of "ein 'gewisser Historismus' des Gaius") to legal studies and who brought together international and natural law ("der erste Jurist, der das ius gentium als ius commune mit naturrechtlichen Auffassung in Verbindung bringt"). Above all he was the first who, with the assistance of Greek philosophy and rhetoric, transformed the parochial *ius civile* of Rome into a theoretical science of law; and he accomplished this especially through the conceptual devices of the *ius gentium* and *naturalis ratio*.This book is a very *anmerkungswissenschaftlich* historical and philological examination of this aspect of Gaian jurisprudence and its socioeconomic as well as legal significance.

In general, according to Wagner, the origins of the *ius gentium* were economic. They must be sought in the ad hoc rules governing exchange (*ius commercii*), secondarily warfare (*ius belli*), between Rome and its accumulating provinces. More fundamental than an imperial order, in other words, was the establishment of a Mediterranean economy for the emergence of a "law of nations." The process began with the admission of foreigners (by the *ius peregrinorum*) to Roman citizenship by legal fiction and so to guarantee basic institutions of sale, lease, and contract (*emptio venditio*, *locatio conductio*, and consensual *societas*). Gaius's contribution was to provide rational order for this economic behavior. As the Greeks had distinguished between *physis* and *nomos*, so he distinguished between national law (*jus proprium*, for the Romans the *ius civile*) and common law (*ius commune*, or the *ius gentium*), which was based on "natural reason" and so identified with natural law (*natura id est iure gentium*). Through the concep-

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tual gifts of the Greeks, in short, the Roman *homo economicus* (and *homo bellicosus*) was transformed into *homo politicus*.

Despite the Marxian interpretation, Wagner's principal concerns are exegetical; "Nun zu den Texten," he proclaims. After a survey of the pre-Stoic and Stoic background, he proceeds to an exhaustive explication of Gaius, *Institutes*, 1, 1, and an analysis in all contexts of the key elements of Gaian terminology, besides the *ius gentium* and *ius naturale*, *consensus omnium*, *naturalis ratio*, and *naturalis aequitas*. In general, Gaius was a transitional figure, not so much a systematic philosopher as a legitimizer of Roman custom as it developed in an imperial economy. For him "natural reason" meant little more than common sense and a way of establishing general (i.e., extra-Roman) validity for economic relations, most notably the institutions of property and exchange called *naturales adquisitiones*. The expansion of universal civil rules into a theory of "natural law" in a modern sense was due to later jurists, such as Ulpian, for whom the *ius naturale* applied not only to bourgeois *liberi homines* but also to other forms of inferior life (*quod natura omnia animalia docuit*). Gaius was concerned with elaborating the rules of private law through *civilis ratio*, as he was the rules of international law through *naturalis ratio*; but his efforts stopped short of speculative philosophy and remained within the bounds of legal and social science. For both these contributions and for his role as Roman social ideologist, this book is a thorough and illuminating guide.

DONALD R. KELLEY
University of Rochester

LÁSZLÓ VÁRADY. *Die Auflösung des Altertums: Beiträge zu einer Umdeutung der Alten Geschichte.* Budapest: Akadémiai Kiadó. 1978. Pp. 148. $13.00.

Four years ago the author of this slim volume published his massive study on the last century of Pannonia, *Das letzte Jahrhundert Pannoniens* (1975). László Várady is also known for his numerous scholarly articles on the problems of the late Roman Empire, all written in his native Hungarian, or German and English. His present study is no disappointment. Várady writes as an experienced scholar in full control of both primary and secondary sources.

His topic centers on the downfall of the western Roman Empire in 476. In the first chapter he concentrates on economic and social questions and here the issue of "servilisation" has his main interest. "Servilisation" is defined as "a complex of economic, political, and cultural relationships of the cultural centers and the barbaricum" (p. 146). Várady sees this "servilisation" as a major cause for the dissolution of Rome. The second chapter reviews the modern Western (non-Communist) studies on the problems of the "fall of the Roman Empire" and the "end of the ancient world." Várady treates these events as inevitable phenomena of the growth and "social realization" of Roman civilization, in which a bankrupt system was replaced by the assimilation of a hitherto excluded "barbaricum" (pp. 49–50). This chapter contains reviews and criticisms of the well-known hypotheses of such scholars as Rostovtzeff, Walbank, Dill, Piganiol, Baynes, Kornemann, Altheim, Boak, Mazzarino, Jones, Vogt. In the same chapter there is also a section on the boundaries between "ancient" and "medieval" history, and here, too, the views of a number of modern scholars are reviewed and criticized. This chapter ends with a brief discussion of what the Romans themselves thought about the eventual dissolution of their empire. The final, third chapter concentrates on the western empire. The author first deals with the solidarity of east and west and its gradual breakdown, in the face of barbarian invasions and other difficulties. The next section examines the division of the empire in 395 in the light of contemporary sources, and here the particularly vulnerable position of Illyricum is given close attention. The following section deals briefly with the earlier division of the empire in 364 between Valentinian I and his younger brother Valens; the author seeks to find out whether this division is the juristic basis for the later division of 395. The last section deals with the events of 476 as the last consequence of the division of 395. A brief summary and an extensive bibliography end the book.

This study is interesting reading, especially so for those who wish to learn more about the Marxist-Leninist interpretation of the dissolution of the Roman Empire.

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MEDIEVAL

DAVID C. LINDBERG, editor. *Science in the Middle Ages.* (Chicago History of Science and Medicine.) Chicago: University of Chicago Press. 1978. Pp. xv, 549. $40.00.

Over the past two decades, the ranks of scholars specializing in medieval science have grown significantly, as has the amount of secondary literature that must be mastered by anyone interested in this segment of the history of science. It is therefore not only appropriate but indeed welcome that one of the most active scholars in the field today, David C. Lindberg, has asked a number of colleagues to pro-

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vide for beginning students and other scholars surveys of the present state of their respective fields.

How to organize such surveys always presents problems when the intellectual activities of more than a millenium (400–1500 A.D.) and several cultures (ancient Greece and Rome, Islam, and early and high medieval Europe) are covered. In this case, organization by disciplines has been adopted, with individual chapters devoted to mathematics; the sciences of weights, motion, and matter; cosmology; astronomy; optics; medicine; and natural history. Six contextual chapters—on early medieval science, the transmission of Greek and Arabic learning to the West, the philosophic and institutional settings, the classification of the sciences, and magic and science—round out the coverage.

The approach that each author takes in surveying a portion of the whole provides an interesting commentary on the diversity and richness of medieval science. For some (for example, James A. Weisheipl on classification and Joseph E. Brown on the science of weights), the ancient backdrop to medieval developments is the necessary point of beginning and the framework within which later developments must be placed. For others (for example, Lindberg on optics), Islamic developments require more space, thereby reflecting their importance. The main focus of the book, the period after 1200, is characterized differently by each author. The chapters on cosmology (Edward Grant) and the science of motion (Edith D. Sylla and John E. Murdoch) focus on northern Scholastic developments of the fourteenth century. William A. Wallace (on the philosophic setting), Brown, and Weisheipl lay much more stress on the thirteenth century, while also focusing on the northern Scholastic tradition. Understandably, Charles H. Talbot's excellent discussion of medieval medicine places less emphasis on northern and more on Italian developments. Talbot and Olaf Pedersen (on astronomy) also extend their surveys to include the much-neglected fifteenth century, whereas Bert Hansen's discussion of magic draws a significant portion of evidence from sixteenth- and seventeenth-century thinkers. Scholastic science plays a lesser role in Michael S. Mahoney's, Robert P. Multhauf's, and Pedersen's discussions of mathematics, chemistry, and astronomy, largely because these sciences had important institutional settings outside the universities.

The lesson that can be drawn from the richness of the approaches taken is that there is no single handle that can be grasped to gain control of the science of the premodern era. As Lindberg correctly notes, medieval science was not "an autonomous discipline or an identifiable profession" (p. xiii). Rather, it ranged broadly, joining here with technology (for example, in medicine and astronomy), there with theology, here with philosophy and metaphysics, there with popular beliefs and superstitions. The student interested in learning more will find that nothing short of cover-to-cover reading will suffice to survey the field.

The reader who does attempt to stay the distance may come away frustrated on two accounts. First, the topics covered exclude one important aspect of the scientific enterprise, methodology. This topic has received some attention in the past (by Crombie, Randall, Wallace, and others) and is a minor theme that runs through many of the articles. However, this minor theme is all but drowned out by the corresponding major theme, the content of the individual sciences. Certainly, anyone who uses this book to initiate classroom discussion may find methodology an interesting place to begin. The second frustrating aspect of these articles is the way they fragment the personalities of individual scientists. The writings and thoughts of such important figures as Boethius, Aquinas, Ockham, and Buridan are so compartmentalized that they and other figures seldom emerge as the complex and interesting persons they were. In one respect this is unfortunate because in the process one important historical feature of medieval science, the universal orientation of its practitioners, is replaced by a feature of modern science—specialization—that few medieval scientists anticipated. On the other hand, it is difficult to imagine, given the state of present scholarship, how else the volume could have been organized. These solid articles with their excellent bibliographies reflect quite nicely the considerable effort that has been undertaken in making the study of medieval science a sophisticated historical endeavor. If there is more work that needs to be done, this is as it should be.

NICHOLAS H. STENECK
University of Michigan, Ann Arbor

JEAN RICHARD. *La papauté et les missions d'orient au moyen age (XIII^e^–XV^e^ siècles).* (Collection de l'École Française de Rome, number 33.) Rome: École Française de Rome; distributed by Boccard, Paris. 1977. Pp. xxxiv, 325.

As the title indicates, this work is essentially a study of the Roman Catholic Church's missionary activity in the late Middle Ages, with special emphasis on the role the papacy played in this medieval attempt to expand Christianity. The text is organized topically and chronologically into five sections: (1) Missions from the Appearance of the Mendicant Orders to the Mongol Conquests; (2) The Mongol Conquests and the Missions; (3) The Birth of a Missionary Episcopate and the Missions to the Mongol

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Empires in Northern Asia, in Central Asia, and in the Far East; (4) The Archdiocese of Sultanieh: the Missions in Persia and the Armenian Union; (5) Missions in the Black Sea and Caucasus Mountain Regions and the End of the Medieval Missions. This detailed study is followed by a lengthy conclusion that is particularly helpful in tying together all the specific material presented in greater depth earlier in the book.

The medieval mission had a twofold purpose. In addition to converting pagans and infidels, it also wanted to reunite with Rome all of the variant Christian communities found in Asia. Information on the medieval mission comes from fragmentary sources; interpretations and conclusions are admittedly colored by lack of adequate sources. Despite this limitation, the historian must evaluate his evidence and come to conclusions. Jean Richard does not shirk his duty. There were significant areas in which the medieval mission was not successful: it failed to convert the Mongols to Christianity, to unite all Eastern Christian groups with Rome, and to make enough converts to sustain some of the newly established ecclesiastical provinces. These shortcomings were not necessarily the fault of the medieval mission itself; they must be viewed in conjunction with outside factors like the Black Death, which resulted in a manpower shortage, the Avignon residency and Great Schism, which diverted papal attention to internal matters, the conquests of Tamerlane, which created turmoil and chaos, and the rising Ottoman Empire, which joined the Mamluks in blocking Western access to the East.

Although the medieval mission had only limited success in achieving its two primary goals of converting nonbelievers and reuniting Eastern Christians with Rome, it did make some noteworthy contributions to European religious and intellectual development. Negotiations with variant Eastern Christians were responsible for the emergence of a papal policy of ecumenism. Missionaries began to learn Asian languages and thus were to play a major role in the translation and exchange of intercultural ideas. Their travels to far-off places gave European society eyewitness exposure to the true size and scope of world geography and to different cultural beliefs, patterns, and values. Although one cannot speak of success or failure in absolute terms, these overseas missions were an important part of medieval Europe's growing geographical knowledge and intercultural awareness. Thus, Christian European society was better prepared to take in stride the age of exploration and discovery that followed.

This volume is a typically well-researched and well-documented French monograph with an impressive scholarly apparatus. The inclusion of eight maps is especially helpful in locating the medieval place names no longer in use. The bibliography is complete and up-to-date; it devotes more than the usual space to precious primary source materials, particularly those dealing with the Holy See, since papal documents are the chief source upon which this study rests. Richard's artistically orchestrated use of all pertinent original sources clearly illustrates his familiarity with all of them and with all appropriate secondary literature. The author is well qualified to write this volume due to his past research and publications dealing with the Crusades, Mongol contact with the Christian West, and European trade with the Levant, as well as with the papal involvement in all of these activities. While others have dealt with various aspects of overseas medieval missionary activity, no modern scholar has attempted to present the complete overview Richard has undertaken here. In short, this book is an excellent piece of serious historical research and scholarship; it is a literary contribution that will not soon be replaced or surpassed.

GREGORY G. GUZMAN
Bradley University

ALEXANDER MURRAY. *Reason and Society in the Middle Ages*. Oxford: Clarendon Press. 1978. Pp. xiv, 507. $39.00.

Alexander Murray's intriguing book rests on several theses. First, since about 1000 two poles have attracted the European mind in its search to understand why human life is less than it could be: the "Greek" trust in reason, with the corollary faith that evil derives from ignorance and is remedied by learning; and the "Hebrew" resignation to human willfulness, which prevents people from always doing right, even when they know what it is. "Greek" nature behaves with lawlike regularity accessible to human understanding and even to human exploitation. "Hebrew" nature lies open to divine intervention, to be suffered and witnessed but not comprehended. The former view fits a secular, open society; the latter, a religious, traditional order. Second, between roughly 1100 and 1400 European culture shifted decisively toward the "Greek" pole, placing new emphasis on both reason and reasonableness in its understanding of nature, man, and society. Third, although the new presence of Greek learning in the twelfth century was a necessary condition of the shift, it was not sufficient in itself to cause such a reorientation of worldview. Rather, a new socioeconomic and political reality made wealth and status especially accessible to those who grasped that knowledge is power. Beginning in the eleventh century, the spread of money made wealth more liquid and, with it, society more fluid; as wealth became abstract, so did its power, now embodied in institutions rather than individ-

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uals. The future lay with those who knew how to count wealth and administer institutions.

Murray's analysis neither relies on nor espouses any mechanism of linear cause and effect. Instead, he assembles a collage of phenomena into a pattern to be viewed as a whole before being examined in detail. The argument is complex and begins with money: when, how, and to what extent it came into circulation; how it made wealth mobile. Murray then examines money's social effect by tracing new patterns of acquisitiveness: avarice toward wealth, ambition toward status and power. Money set the goal; intellectual tradition and practical experience suggested the means of reaching it, reason.

Part two follows the development of one form of reason, arithmetic. Its tools and techniques are perhaps less important than the new mentality they reflect. Before Europeans could learn to do arithmetic better, they had to think to use it at all, to become numerate as well as literate. Skill in reckoning acquired recognized social utility; those who possessed it took over effective control of economic and political life. As part three shows, reckoning was only one of the "lucrative sciences" taught at universities which were founded on the premise that reason is power and which turned out an intellectual elite whose self-consciousness claimed autonomy from the political and religious institutions its self-interest fed on. Behind fourteenth-century assaults on political privilege and clerical illiteracy (the latter, at least, an imaginary target) lay the career anxieties of learned men in Europe's first crisis of academic over-supply. As a final contrast in this juxtaposition of positive and negative images, Murray explores noble piety and the phenomenon of the medieval saint who, though usually of noble birth, become "socially amphibious" and held universal appeal.

Murray's strategy of arguing a broad thesis from scattered fine details makes his book both stimulating and puzzling. How does one differentiate criticism from cavil? For example, his discussion of arithmetic ignores the classical and medieval distinction between arithmetic (the science of numbers) and logistic (the art of reckoning). Failing to appreciate how systems of numeration fit techniques of calculation, he inverts the historical relation of abacus and algorism. Convinced of the inherent superiority of decimal place-value reckoning, he sets up as a paradox the retention of Roman numerals and the abacus by otherwise "progressive" men. But there is no mystery. The abacus is faster than written calculation and takes up no paper; Roman numerals betray tampering. The paradox is specious.

These are substantial flaws. Yet, the argument of increased numeracy still seems right. Is the detail here unnecessary, then? If so, how about elsewhere in the book? One must read *Reason and Society* actively, challenging it in response to its challenges. The reward may lie more in the process than in the product.

MICHAEL S. MAHONEY
Princeton University

RENÉE DOEHAERD. *The Early Middle Ages in the West: Economy and Society.* Translated by W. G. DEAKIN. (Europe in the Middle Ages, Selected Studies, number 13.) New York: North-Holland. 1978. Pp. xvii, 307. $31.00.

This work is a translation of *La haut Moyen Age: économies et sociétés* (1971), originally published in the "Nouvelle Clio" series. It deals with the early "medieval economic structure" from the fifth through the tenth centuries in the areas of Europe controlled by the Frankish monarchy. The principal chapters deal with "the scarcity of production" and demography, agricultural production, "the circulation of consumer goods," and the problems of currency, prices, and credit. Despite the title, little attention is paid to social structures, to groups such as the nobility, peasants, women, and children, or to concepts such as kinship, gift-exchange, and honor.

In her analysis Renée Doehaerd assumes that the economy of the early Middle Ages was permanent and largely unchanging and that such changes as occurred over some five centuries were not "sufficiently important to transform [human activities] or to alter people's lives" (p. 263). But the levels of economic development, or even of basic subsistence, did differ substantially from place to place and changed over time perhaps as much as in any other five-hundred-year period of European history before the Industrial Revolution. To hold, as the author does, that the early Middle Ages was a period of essentially unchanging and undifferentiated scarcity comes perilously close to resurrecting the myth of the "Dark Ages." Nonetheless, these assumptions allow the author to use numerous examples to illustrate particular points with little regard to time or place: for example, passages from Gregory of Tours and Peter Damian are used to illustrate the acquisitive activities of the great landowners (pp. 89–90), and both Pope Gregory I and Lupus of Ferrières are cited on their concern for the poor on their estates (p. 7). In fact, the whole book is largely a collection of such examples.

Doehaerd does make some important interpretive points. She notes, for example, the serious problems in using the polyptychs as sources of demographic information, especially since they in no way constituted complete censuses. But the book's broad analytic structure is neither very original nor always logically sound. One cannot explain scarcity by "demographic weakness" and then argue that low

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population was the result of scarcity without leaving the impression of logical circularity. Doehaerd does offer another explanation for scarcity that is both more interesting and less discussed, that early medieval landholders invested all too little intellectual leadership and material resources in agricultural production and distribution. But R. Latouche's *The Birth of the Western Economy* (2d ed. 1967) and G. Duby's *The Growth of the European Economy* (1974) cover much the same ground as Doehaerd; the former is more tightly structured, especially chronologically, and the latter is more provocatively argued.

In addition to the shortcomings attributable to the author, the editor and publisher have contributed far too many of their own. The translation into English is reasonably straightforward, though certain infelicities remain (for example, "vassalhood" and "upper Middle Ages"); moreover, a large number of quotations are left in Latin. But as it now stands the scholarly apparatus—bibliography, notes, and index—is wholly inadequate. Besides numerous incomplete citations, failure to cite the best or most recent editions, and outright errors, the bibliography is so out-of-date that very few post-1965 works are listed, only two after 1966. Moreover, though the English translations of half a dozen works are mentioned, this is not the case for at least forty others for which translations are available. In the original French edition the several sections of the bibliography were individually alphabetized, but the whole was numbered consecutively. In the English version, however, the numbers have been inexplicably lost; thus, the notes are nearly useless, since they still appear in the author/short-title form but now lack the number that would allow the reader to locate the reference in the bibliography. Given Doehaerd's exemplificative approach and her over one thousand three hundred and fifty notes, this is a particularly grevious editorial omission. Nor is the index very useful. For instance, I counted over forty references to slaves and slave-trading that are not indexed. English-speaking students of medieval history should perhaps feel grateful to North-Holland for publishing translations of many important secondary works originally written in French and German. They would be more grateful if the level of editorial care and of usability in the apparatus of works such as Doehaerd's matched the level of the price. The editor and publisher owe an apology to both the author and her potential readers for crippling a quite useful book.

RICHARD R. RING
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HENRY KRAUS. *Gold Was the Mortar: The Economics of Cathedral Building.* Boston: Routledge and Kegan Paul. 1979. Pp. xv, 292. $30.00.

Cathedral building was certainly one of the most visible indicators in the Middle Ages of the allocation of economic resources. To illustrate its significance, it has been estimated that Chartres, built by a minor bishopric in a third-rate town in the thirteenth century, would cost $50 million to reproduce in 1972. Despite the fatuous title, this book is a serious inquiry into the economics of cathedral building. On the basis of eight cathedrals (six French, one Rhineland, and one English) to which he devotes separate chapters, Henry Kraus has traced the patronage of cathedral construction by churchmen, townsmen, local lords, and kings. Equally important, he also has explored the alternatives to such investment provided by local parish churches, landed estates, and crusades. He places all of these factors within the context of local political struggles that dominated the histories of these architectural achievements.

His samples were selected to show the varieties of solutions that were designed. The magnificent edifices of Amiens, Strasbourg, and Rouen were the proud successes of bourgeois investors who were welcomed by ecclesiastical authorities. On the other hand, the glories of Paris and York were almost entirely produced by aristocratic churchmen. Excluded from these enterprises, the townsmen lavished their money on parish churches. Where prelates and townsmen came into conflict, the cathedrals suffered. This is demonstrated by the incomplete Toulouse and the modest Lyon. At Toulouse the cathedral was surpassed by Saint-Sernin and the Jacobins, and at Lyon it was rivaled by Saint-Nizier and the Rhone bridge. The cathedral at Poitiers, falling victim to royal Capetian conquest, exploitation, and preoccupation with the crusades, never achieved distinction, and the Romanesque churches of the region were not rebuilt.

To make these intriguing comparisons, Kraus has asked the right questions. To provide the answers, he has both relied on local monographs and added his own contributions from the extant sources. He has combed the cartularies and other archival collections (claiming, for example, to have consulted the great collection of Dom Fonteneau at Poitiers), collated thousands of wills of townsmen, and studied donors' panels in stained glass windows. Despite these efforts, it is nonetheless difficult to master the sources and historical background of eight disparate cathedrals. He has undoubtedly made mistakes that will annoy specialists, and he argues too frequently from silence without understanding the conditions of the documents. But he has produced a daring study of this important problem that should both inspire and provoke others to carry on the work.

JOHN W. BALDWIN
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I. S. ROBINSON. *Authority and Resistance in the Investiture Contest: The Polemical Literature of the Late Eleventh Century.* Manchester: Manchester University Press, and Holmes and Meier, New York. 1978. Pp. x, 195. $32.50.

The title of this book accurately describes its contents, a study of the pamphlet war waged in the late eleventh century between the supporters of the secular power and those of the papacy. The introduction gives a brief history of scholarly definition of the "Investiture Contest," a survey of the surviving polemical literature, and a good summary of the political situation in the Empire at the beginning of the controversy. I. S. Robinson follows recent scholars like H. E. J. Cowdrey in seeing the eleventh-century reform as "essentially an attempt by men trained in the monastic discipline to remodel Church and society according to monastic ideals" (p. 2).

The first two chapters consider the polemic in behalf of Gregory VII and Henry IV. A new advance is made in showing the degree of Gregory VII's indebtedness to the thought of Gregory I. As interesting is the portrayal of the manner in which, beginning in 1084, the "schismatic cardinals" turned the fruits of Roman canonical research against Gregory. Robinson's judgments on many matters of detail seem to me sound: hence Petrus Crassus's use of Roman law extracts proves neither that Crassus was Ravennese nor that there was a law school at Ravenna.

The next two chapters deal with the literature for and against the *regnum.* Robinson shows the extent to which canonical texts originally intended to describe the powers of the episcopal office were used by Henrician polemicists to define royal rights. The ambiguity in the Gregorian ideas about kingship, which fluctuated between seeing royal power as originating in greed and conceiving the king as *typus Christi*, is explored against the background of assumptions about kingship held by both sides in the dispute. The final chapter considers the role of local issues in determining the polemic of those imperial bishops who were caught between king and pope in their allegiances. An epilogue considers two twelfth-century Bamberg codices on investiture.

My criticisms of this well-crafted book are minor and involve mostly questions of perspective. The term "monastic" is used so loosely in the opening pages that a monastic source could be found for almost any eleventh-century ideal. In virtually every case, one could as easily speak of a patristic source, but rarely does this get at what was new in eleventh-century thought. By concentrating on the question of "authority and resistance," Robinson tends to orient his materials in a way that oversimplifies the issues: thus, his comment that "this was the crux of the debate about the *regnum* . . . not what was its function . . . but whether rebellion against the *regnum* was ever justified" (p. 121). The discussion of the distinction between "office" and "person" would have profited by reference to long-standing differences between Roman and Germanic law. The author is unlikely to be accused of pro-Gregorian sympathies, but of a number of typographical mistakes the only one with possibly Freudian undertones is the reference to the "peach of the Church" under Henry IV (p. 92, read "peace").

GLENN W. OLSEN
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KENNETH M. SETTON. *The Papacy and the Levant (1204–1571).* Volume 2, *The Fifteenth Century.* (Memoirs of the American Philosophical Society, number 127.) Philadelphia: The Society. 1978. Pp. ix, 580. $35.00.

It is a pleasure to welcome this second volume in Kenneth M. Setton's magisterial history, *The Papacy and the Levant*, one that deals with the fifteenth century or, to be more precise, with the years from 1402 to 1503. It again provides the scholarly world with a rich narrative based upon an incredible mass of archival and secondary materials. Written, as the author tells us, largely from the standpoint of how events were perceived in Rome and Venice (especially the latter), it tells the sad story of the failure of the Latin West to stem the Ottoman Turkish advance in the Near East down to the death of Pope Alexander VI in 1503. In doing so, it explains how rivalries in Italy proper, general European disunity, papal weakness, and the Latin-Byzantine inability to cooperate contributed to the Turkish advance on land and sea and assured the even greater victories that Selim the Grim and Suleiman the Magnificent were to enjoy in the next century.

It is interesting that the author only devotes one-quarter of his narrative to the half century before 1453 and that he tells the story of the final fall of Constantinople in only thirty pages. Where he is especially detailed is in his handling of the abortive efforts of Popes Pius II, Sixtus IV, Innocent VIII, and Alexander VI to launch successful crusades against the Ottomans. One is also struck by the way he shows us an Alexander VI who made serious efforts to unite Christendom against the Turkish menace.

It is difficult to find much one would change in this volume. Some might wish more had been offered us concerning Hungary and Eastern Europe in general—though careful reading of chapters four and six, which deal with the Crusade of Varna and events in Serbia and Albania respectively, show that the author has a thorough mastery of the mate-

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rials of Eastern European history. It is also perhaps regrettable that so little attention is paid to the final stages of the Reconquista in the Iberian peninsula, which led not only to the fall of Granada but also to considerable Portuguese, Castilian, and Aragonese naval activity in the western Mediterranean and along Maghrebi shores, which in turn had some impact upon Italy and the Near East. One also wishes at times that more attention had been paid to the flow of commerce from Alexandria and Beirut to the West, and to Italy in particular, and the way this trade affected the Near East in general.

These, however, are minor caveats. What is important is that we now have a monumental and definitive study of a crucial century in East-West relations in the Levant that explains in detail how and why the Ottomans were able to take over the Near East with very little real Latin Western opposition, although Western Europeans had much to lose from this expansion. We all await eagerly the author's final volume, which will carry the story down to 1571.

ARCHIBALD R. LEWIS
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CLARENCE GALLAGHER. *Canon Law and the Christian Community: The Role of Law in the Church According to the Summa Aurea of Cardinal Hostiensis.* (Analecta Gregoriana, number 208.) Rome: Università Gregoriana Editrice. 1978. Pp. 242. L. 9,000.

As the subtitle indicates, this volume of the Analecta Gregoriana, a 1970 dissertation for a doctorate in canon law, investigates the attitude of Cardinal Hostiensis (d. 1271) toward canon law and its functions in the life of the Church. Hostiensis's *Summa super titulis decretalium*, or *Summa Aurea*, is chosen as representative of medieval canonical thought in general. Clarence Gallagher undertook his inquiry as a contribution to contemporary debates about the role of law in the Church of today. In other words, the volume is not intended primarily as a contribution to historical scholarship.

This theme is delineated in the introduction. Chapter one provides a biographical introduction to Henry of Susa, Cardinal Hostiensis, and chapter two gives a very basic historical introduction to canon law before Hostiensis. The most interesting, but extremely brief, chapter three (pp. 65–85) discusses Hostiensis's general attitude toward the role of law. Chapters four to six present and sometimes analyze specific legal views as developed in the detailed *Summa Aurea*, addressing such subjects as the relationship between Church and state, papal primacy, justice and equity, guidance for Christian living, and legal punishment as a deterrent. Chapter seven provides a summary, perhaps of necessity a tedious and repetitious section, as well as an evaluation of Hostiensis in the light of modern thought. In general, Gallagher is led to conclude that "the canonist's role in the Church is to provide the precision of thought, the accuracy and lucidity in language which are needed if theological truths are to be effectively reduced to action within the community" (p. 219).

It would be difficult to find fault with this general statement, but it hardly satisfies a historian who cannot suppress some uneasiness at Gallagher's utilitarian approach to medieval thought. Far be it from me to deny the usefulness of historical study, but the Middle Ages and 1970 cannot be confronted in a one to one relationship as this book attempts to do. The usefulness of history is indirect; it reduces contemporary problems to accurate proportions by providing a long-range view and perspective. It is this historical perspective that is largely missing in the volume despite the brief and very basic introduction to Hostiensis's juridical background, where one reads, for instance, that Gratian's *Decretum* was "published in 1141" (p. 50). Gallagher's interesting attempt to present a straightforward account of Hostiensis's view of the role of law is therefore not completely satisfying.

Despite some obvious limitations of the book (among them, the intentional omission of more than incidental references to Hostiensis's original contribution to canonical thought [p. 212]), it will be found very useful as a general introduction in English to Cardinal Hostiensis and his *Summa Aurea*. It goes beyond dictionary entries without overburdening the general reader. The volume performs a further service by introducing readers to some secondary literature in English that deals in greater depth than Gallagher's own book with particular aspects of the work of Hostiensis, for example his contribution to conciliarist thought. Incidentally, Gallagher states, without probing very deeply or adding to the discussion of the problem by Brian Tierney, J. A. Watts, and Alfons Stickler, that "the author of the *Summa* does not reveal conciliarist tendencies" (p. 112). Anyone who is not exclusively concerned with Hostiensis's attitude toward the role of law would have to follow up the bibliographical references found in the secondary works on which Gallagher relies to obtain a fuller understanding of Hostiensis, but this does not detract from the basic merit of this book, an introductory monograph on one of the greatest figures among the medieval jurists.

UTA-RENATE BLUMENTHAL
Catholic University of America

DAVID A. HINTON. *Alfred's Kingdom: Wessex and the South, 800–1500.* (History in the Landscape Series.) Lon-

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don: J. M. Dent and Sons. 1977. Pp. xii, 228. $12.50.

The gap between history and archeology is wider than most lay readers realize. The study of the artifacts and material remains of a past society is usually well removed from the analysis of its written ideas, self-conscious reflections, and public and private records. While archeology has tended to overemphasize or isolate the physical and corporeal, history has leaned toward the intangible and didactic. The bridge is hard to build, and it was the great pre-historian, V. Gordon Childe, who reminded us that archeology could give us the bath tub but not Archimedes.

Recently a number of English and British archeologists have tried their hand at synthetic books that sum up recent work—mostly on Anglo-Saxon and early medieval life—and at integrating such data with what we know about this world from familiar written sources. With this volume David A. Hinton joins the useful company of such authors as D. M. Wilson, David Brown, and Charles Thomas. Hinton begins with several chapters that analyze the physical achievements and remains of Alfred's West Saxon kingdom. He then proceeds to devote chapters to such topics as the medieval church and towns, trade, and transport. He concentrates on the south, from the southern midlands to the south coast, and his main period of interest runs from about 900 to 1300.

The volume is of high but uneven quality. The problem of allowing the quantity and quality of the physical remains to govern our comments is that these remains are neither uniformly preserved or evenly knowable. When the subject is well served by archeological digs and publications, Hinton is detailed and comprehensive. His comments on the remains of Hamwic, Southampton's early predecessor, or on the evidence that we can derive from the Burghal Hidage are models of that elusive link between documentary and unearthed history. His chapter "Town and Country Buildings" contains a mine of information on styles and building methods, on the evolution of carpentry, and on local tile trade. Few of us are so strong on "cruck built" houses that we can fail to profit from the relevant text and illustrations. But the various chapters are not related to each other, and here and there a section gives the impression of being slightly padded.

In summary, the book is a collection of excellent short essays, with only a limited overall unity. It has several strengths I would like to note. One is that Hinton neither glosses over the shortcomings of archeological technique and knowledge, nor does he ignore the many digs that have yielded nothing of value. He is quick to acknowledge the difficulties of dating and of interpreting. Another strength lies in the use of the forty line figure drawings and the twenty-nine plates to drive home points, both general and (especially) technical in nature. Whether we are looking at a plan of Wareham, or at a selection of models of floor plans for peasant huts, or at a diagram of a forty-foot-high barn, we have complicated material reduced to a level we can replicate to our students. A current bibliographical essay rounds out the book. One humorous touch: Hinton, as an experienced and productive archeologist, enjoys telling us when he counseled against a dig that proved most productive and when he argued for one that turned out to be a dud. Archeological books often have a strong autobiographical touch, and the personal asides are both amusing and perceptive in their warning against hubris and historical certainty.

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THOMAS D. O'SULLIVAN. *The De Excidio of Gildas: Its Authenticity and Date.* (Columbia Studies in the Classical Tradition, number 7.) Leiden: E. J. Brill. 1978. Pp. 200. f 60.

This small book is a fine exercise in revised revisionism. Its goal is the resolution of current debates about the *De Excidio*, a unique document apparently authored during the years of transition between periods of Romano-Celtic and Saxon dominance in Britain. Virtually everything about the *De Excidio* and its reputed author, Gildas "the Wise," has at one time or another been called into question. Thomas D. O'Sullivan sifts through the arguments and concludes that scholars have been excessively suspicious of the authenticity of Gildas's work. He does not undertake a new edition of the Gildasian manuscripts, and those who seek a full Latin text or modern translation of the *De Excidio* should not misread the title and search for it here.

O'Sullivan's subject is the history of the interpretation of the text. Doubts about the unity of the *De Excidio* go back at least to the seventeenth century, when the sponsors of its first printed edition decided to publish it in separate sections as "historia" and "epistola." In the early nineteenth century questions were raised concerning the authenticity of the text in any form and the historicity of its author. The school of skeptics found its most effective spokesman in an early twentieth-century scholar, Arthur W. Wade-Evans, whose theories form a major target for O'Sullivan's critique.

Many of the arguments for and against Gildas are, of necessity, based on tenuous inferences from scant data. In each chapter of the book O'Sullivan develops a more or less independent point that,

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when taken with its neighbors, forms a mosaic of probabilities supporting faith in the assumption that the *De Excidio* is indeed what it was first thought to be, the unified work of an authentic sixth-century Briton. O'Sullivan amends standard interpretations of his text by suggesting that it is the work of a relatively young man (twenty-five to thirty years of age) and that the traditional date of its composition (ca. 547) should be revised downward (515 to 530).

There are a few petty imperfections in the book that detract from the pleasure, if not the profit, of reading it. The work is the author's doctoral dissertation defended at Columbia University in 1973, and, despite the labor of reworking it during the tenure of a Mellon Fellowship at the Catholic University of America in 1976–77, it still reads like a dissertation. There is an excessive use of the authorial "we." The defensiveness of the wise servant of an examining committee comes through in O'Sullivan's self-conscious warnings to his readers that the objections or questions which might be forming in their minds will be dealt with in due course (pp. 17, 29, 51). Since O'Sullivan updates his bibliography with the addition of five articles published after 1973, it might have been worthwhile adding a discussion of a sixth. K. M. Martin's "The Adventus Saxonum" (*Latomus* 33 [1974]: 608–639), presents an argument which might be used by O'Sullivan to narrow the range of probable dates for the composition of the *De Excidio* by another five to ten years.

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P. H. SAWYER. *From Roman Britain to Norman England.* New York: St. Martin's Press. 1978. Pp. 294. $19.95.

Though basically a survey, this is a learned, stimulating, and refreshingly novel book. P. H. Sawyer sets out to interpret early English history in the light of recent research; he takes into account the discoveries over the last three decades by numismatists, archeologists, linguists, and social anthropologists, as well as the brilliant contributions of a new generation of Anglo-Saxon historians—David Dumville, Kenneth Harrison, Molly Miller, Janet Nelson, Alfred Smyth, Ian Wood, and Patrick Wormald, to mention just a few of the frequently cited names. Sawyer's volume serves not so much to supplement Frank M. Stenton's standard *Anglo-Saxon England* (1st ed., 1943; 3d ed., 1971) as to offer alternative views on a number of topics.

About half the book, in two long chapters, looks at the main developments from the fourth century to the eleventh; there are illuminating sections on sources for early English history and on Roman Britain, the Anglo-Saxon conversion, and the making of a united kingdom (the latter takes us briskly from the Mercian hegemony through the Vikings to the West Saxon empire). Succeeding chapters analyze social organization, the changing character of kingship, the influence of the Church, the history of settlement in England, and the growth of towns and trade that made eleventh-century England one of the most highly urbanized areas of Europe. A final chapter discusses the consequences of the Norman Conquest. In presenting his new material, Sawyer vividly conveys the excitement of discovery.

Readers of this book will find much to admire, much to query; they will always be forced to rethink established positions. Unexpected minutiae, such as that most pre-Conquest sheep bones found by archeologists are from six-year-old animals, lead on to surprising conclusions. The recognition that such sheep were reared for their wool, not their meat, allows Sawyer to venture that England's relative wealth in the eleventh century depended on earnings abroad. He thus supports the attractive hypothesis that the silver withdrawn in quantity by the Vikings in Æthelred's reign, and later by the Norman conquerors, reached England as trade payments, mainly from Germany where a new silver mine was opened in the Harz mountains in the 960s. Sawyer writes with force and imagination and is notably balanced when coping with the Vikings: like a number of distinguished Anglo-Saxons but in opposition to the West Saxon chronicler, he does not regard the invaders as an unqualified disaster; but neither does he see them as "long-haired tourists who occasionally roughed up the natives" (J. M. Wallace-Hadrill's criticism of revisionist extremes). He is also fair to poor Æthelred: where Professor H. R. Loyn (*The Vikings in Britain* [1977], p. 82) echoes the chronicler's loathing for the king, Sawyer insists that the problems faced by Æthelred in defending all England against Viking attacks were far greater than those Alfred had faced (not least because of the prominence in many parts of the country of men of Scandinavian descent) and that later kings reaped the benefits of his efforts.

A bibliographical guide emphasizing recent publications brings the book to a close. *From Roman Britain to Norman England* can be recommended to anyone with the slightest interest in early English history.

ROBERTA FRANK
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INGE-MAREN PETERS. *Hansekaufleute als Gläubiger der englischen Krone (1294–1350).* (Quellen und Darstel-

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lungen zur Hansischen Geschichte, New Series, number 24.) Cologne: Böhlau Verlag. 1978. Pp. xi, 323, DM 74.

One of the peculiarities of English life in the fourteenth and fifteenth centuries was the prominent participation of foreign merchants in managing exports and financing major loans to the government. It has long been the prevailing wisdom that foreign merchants advanced large sums of money to the English government in order to obtain trading privileges.

The major thesis of the present study on Hansard creditors of the crown in the first half of the fourteenth century is that loaning money helped the northern German merchants to go about their business more efficiently. At a time when the king was spending large sums of money in the Low Countries to support an activist foreign policy, Hansard merchants were exporting large amounts of coin to England in order to buy wool and pay the increasingly onerous export duties. When German wool merchants delivered gold or silver to English agents at Bruges, they received documents freeing them from paying a specified amount of the duties on their wool exports. This spared both parties from having to ship bullion across the channel. On the whole, the purchase of trade privileges accounted for a small portion of these loans. It was only after the collapse of the Florentine bankers in 1339 that the Hansards of Dortmund and Cologne became prominent creditors to the crown. During the 1340s, German merchants loaned money in exchange for wool, for credits on export duties, and in the end even for the management of the royal customs and of the tin mines in Cornwall. As the scale of loans rose, the number of individual creditors shrank, until Tidemann Lindberg emerged as the sole agent of the *Hansa Alemannie* in England. The brief era of German dominance ended as quickly as it had begun, with the retirement of Lindberg's loans in the 1350s. The crown now tried to make do with domestic loans and subsidies.

Inge-Maren Peters has demonstrated that the floating of major state loans did not necessarily withdraw money from commerce, since German merchants exploited the loans to help them transfer funds more economically. Further, the old view that the Hansards were financially unsophisticated will have to be revised. Peters has constructed a detailed account of the mechanics of state finance based foursquare on the manuscripts in the Public Records Office. The defects of her study arise from this very obsession with detail: the motives of the English government are never penetrated, and the collapse of the cozy relationship between Lindberg and the crown in 1350 is never adequately explained. Using the book is made more difficult by the lack of an index. The materials have been gathered here for a broader synthesis on Anglo-German relations in the fourteenth century, but the author has clearly left that for others to do.

STEVEN ROWAN
Newberry Library

WENDY DAVIES. *An Early Welsh Microcosm: Studies in the Llandaff Charters*. (Royal Historical Society, Studies in History Series, number 9.) London: The Society; distributed by Swift Printers, London. 1978. Pp. xi, 208. $17.00.

The historiography on early medieval Wales has remained uneven owing to the dearth of primary materials of any substance or reliability. Pre-Conquest literary sources like Gildas's *De Excidio* and Nennius's *Historia Brittonum* suffer from all sorts of critical problems, while non-Welsh narratives like Bede's *Ecclesiastical History*, the *Anglo-Saxon Chronicle*, and others contain only glimpses into early Welsh affairs. Similar inadequacies attend what little we possess of early Welsh official records. Outstanding among these—and epitomizing the problems inherent in sources of its type—is the *Liber Landavensis*, elsewhere described by Wendy Davies as "part-cartulary, part-history, part-register of the diocese of Llandaff." The *Liber Landavensis* conveys, *inter alia*, one hundred fifty-eight charters making it, at least potentially, the most important collection of documents relating to early developments in southeastern Wales. But since its first publication by the Welsh Manuscripts Society in 1840, followed by J. G. Evans's edition of 1893, scholars have wrestled with the authenticity of its charters. Written in a single hand of the mid-twelfth century, the charters have been either branded as later forgeries or accepted as legitimate pre-Conquest documents.

This book both follows from Davies's accumulated research in the *Liber Landavensis* and precedes the appearance of her *Llandaff Charters*. Its purpose is to offer an interpretation of the development of southeastern Wales in the early medieval period by exploiting the full potential of the charter material. The work consists of two parts. The first discusses the numerous problems associated with the charters as sources, as well as the role of the charters in the construction of the *Liber Landavensis*. From an analysis of diplomatic formulae and witness lists Davies concludes that the Llandaff collection had been subjected to various stages of editing before its appearance in the *Liber Landavensis*. This, in turn, suggests the existence of an original and early set of texts and undermines the argument for the charters as late forgeries.

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Part two provides the historian with insights into Welsh economic, political, social, and religious developments. We learn that, contrary to what we would expect from Welsh law, the kindred had little restraint upon the alienation of property. The charters also suggest fundamental social changes sometime in the eighth century: the break-up of larger estates into smaller units and the beginning of grants by the laity. Later centuries saw gradually increasing aristocratic lawlessness to the detriment of royalty—the opposite of contemporary English or West Frankish events. Again, the charters reveal a greater affinity in Welsh ecclesiastical matters with Roman, rather than with Irish practices; episcopal powers appear to have been much greater than abbatial.

Given the nature of the charters, as Davies points out, these conclusions are necessarily inferential. But the charters certainly supplement our knowledge of post-Roman Wales, and they suggest interesting possibilities for future research.

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MARTIN HEINZELMANN. *Bischofsherrschaft in Gallien: Zur Kontinuität römischer Führungsschichten vom 4. bis zum 7. Jahrhundert. Soziale, prosopographische und bildungsgeschichtliche Aspekte.* (Beihefte der Francia, number 5.) Munich: Artemis Verlag. 1976. Pp. 280.

This very complex study addresses a familiar but still unresolved historiographical problem: whether continuity or disruption was the key feature of the crucial era (fourth to seventh centuries) marking the transition from the ancient to the medieval world. The author approaches this issue by investigating a particular body of source material relating to a particular group that played an important role in the history of the period. The prime actors are selected bishops, chiefly from southern Gaul, whose fate is linked for the purposes of this study by the fact that their tomb inscriptions have survived and that, in most cases, this body of material can be complemented by hagiographical and other prosopographical evidence (especially bishops' lists).

As his subtitle indicates, Martin Heinzelmann argues that this evidence demonstrates a remarkable continuity of the ruling class and of its conception of itself from Roman times to the seventh century. After a brief introduction treating certain aspects of his methodological approach, he begins to build his case by describing the literary form and substantive content of the *laudatio funebris* developed in noble circles of the Roman Republic to honor the dead and their families. He traces the persistence of this tradition into the seventh century and argues that it remained almost unchanged except for certain Christian values grafted onto the pagan base to enhance the prestige of society's elite. Then he proceeds to a minute analysis of the tomb inscriptions and biographical data of a group of Gallic bishops, seeking whatever can be discovered about who these bishops were and for what they were honored. This review cannot do justice to the skill with which Heinzelmann uses these difficult sources nor to the immense learning he brings to bear on their elucidation. What he concludes is fairly simple: these Gallic bishops were members of long-established Roman families who viewed their control over the episcopacy as an extension of their right to direct society. And they were honored for the same virtues that the Roman ruling class had for centuries ascribed to itself as signs of fitness to rule, except that, with the passing of time, that class had absorbed Christian ideas into its scheme of values without fundamentally altering the old order of things. In short, the Gallic bishop of these later times was the prime exemplar of a consciousness of nobility whose origins must be sought far back in the Roman world; more significantly, this deep-rooted consciousness of what constituted *nobilitas* was a critical ingredient in the formation of a medieval aristocracy that, because of it, needed neither emperor nor king to provide it with sanction for the exercise of economic, political, cultural, or religious power.

Heinzelmann's study will provide powerful ammunition for those who find continuity of Roman elements as the crucial ingredient of historical development during the era reaching from the fourth to the seventh centuries. Some may argue that he overemphasizes what was said of men on their tombstones. Others may question his assumption that the language of praise used in one milieu carried the same ideological content as the same language used in another setting several centuries later. Certainly there are good grounds for challenging the way in which he explains away the impact of ascetic ideas on late Roman and early medieval concepts of virtue. Nevertheless, this study makes a powerful case for the ideological affinity linking the senatorial noble of the pagan Roman world to the Gallic bishop of the sixth century.

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PETER R. MCKEON. *Hincmar of Laon and Carolingian Politics.* Urbana: University of Illinois Press. 1978. Pp. xiv, 327. $12.00.

Hincmar, bishop of Laon (858–79), was nephew to the great Hincmar of Reims and, like his uncle, a scrapper in the rough-and-tumble ecclesio-political quarrels of the later ninth century. The younger

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Hincmar was impetuous and combative, and his feuding with Archbishop Hincmar and King Charles the Bald, which cost him his see and his sight, was the occasion for an important letter and pamphlet war over issues of ecclesiology. Karl F. Morrison, in his *Two Kingdoms: Ecclesiology in Carolingian Political Thought* (1964) used the quarrels as a springboard for a masterly analysis of the various strands of ecclesiology in the Carolingian realm: papal monism, royal monism, and conciliarism. More recently, Jean Devisse's three-volume work, *Hincmar, Archevêque de Reims, 845–882* (1976), also treated the affair of Hincmar of Laon (pp. 738–90). In each book, Hincmar was treated as a participant in some larger struggle, as a supporting actor in a complex drama. In the book under review, Peter R. McKeon has attempted to focus exclusively on his career as "a study of some of the intricacies involved in the manipulation of political power . . ." (p. xiv). McKeon established a *Registrum Hincmari Laudunensis*, containing one hundred seventy-eight items, primarily letters to, from, and about Hincmar, and on that source base composed his monograph. Hincmar's involvement in the great issues of the day (the divorce of Lothar II, Carloman's conspiracy against Charles the Bald, the disputes with the elder Hincmar and the king) and his involvement in lesser disputes with his vassals and clergy are traced conscientiously and thoroughly, within the limits imposed by the sources.

The resulting book is thorough in its coverage, pedestrian in its prose, and lacking in the deeper analysis that might have revealed the significance of the events narrated. The author disclaims any intention to write a biography, but, in treating one man, he is occasionally drawn to biographical techniques of doubtful value. The scrappy, formalistic sources cannot support insights of the following sort: "Hincmar of Laon did not possess this subtlety and insight. The direction of his actions was so dominated by immediate gratification that he was largely incapable of attributing to others any drives that bore no relationship to himself and his own desires. He saw people as being essentially monolithic, devoid of will and life until so endowed by his attention" (p. 99; cf. pp. 107, 110, and 181). In spite of the detailed analysis of events, there is insufficient attempt to place Hincmar's struggles, large and small, in wider legal or theological frameworks. For instance, although Morrison's book is corrected on chronology and some facts, it is still the place to turn for the *significance* of the quarrels. Finally, there is a question whether this subject warrants treatment in book form. Even with expendable excursuses on geography and brief biographies of participants, it is a rather short book (185 typed pages of text and 140 typed pages containing notes, bibliography, *Registrum*, appendixes, and index) and might better have appeared as a long, carefully edited journal article or articles.

JOSEPH H. LYNCH
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JACQUELINE CAILLE. *Hôpitaux et charité publique à Narbonne au Moyen Âge de la fin du XI^e^ à la fin du XV^e^ siècle.* Toulouse: Privat, for the Société Française d'Histoire des Hôpitaux, Paris. 1978. Pp. 191.

This study of the hospital (or almshouse) and public charity at Narbonne in the Middle Ages was published under the auspices of the Société Française d'Histoire des Hôpitaux and the Fédération Hospitalière de France. This may help account for the fact that the study does not differentiate between institutions for the ill (except, of course, for lepers) and those for the general provision of aid to the unfortunate, the poor, and the traveler. The author refers to care of *les malades* in many of the eight institutions found in the *Bourg* and in the twenty-one that dotted the *Cité*, but she also notes that it was only at the end of the period under investigation, 1488 to be precise, that a physician was named to the staff of any one of these. The author mentions the Black Plague; it visited Narbonne, but there is no discussion of its impact on public policy and charitable institutions. She does not address whether or not Narbonne ever established a true lazaretto on the basis of a grasp of contagion theory. Narbonne was situated close to the university at Montpellier, where the curriculum in medicine emphasized the interdependence of medical practice and academic medicine. But charitable Narbonnais institutions do not appear to have been the arena where these ideas were tested. Where, then, did physicians practice? This study will not suit the needs of scholars interested in the development of communal health policy or the evolution of the concept of adequate medical care.

Instead the study addresses the question of eleemosynary institutions from the late eleventh century onward. It comes as no surprise that there was a complex overlapping of jurisdictions, all parties focusing attention upon the unfortunate. Until the middle of the thirteenth century, concern for the afflicted, and coincidentally for the soul of the donor, appears to have ranked before concern for jurisdictional authority over institutions. At that juncture civil authority achieved primacy among the concerned parties: the archbishop, the Hospitalers, orders of the church, and lay donors. Civil authorities did not drive out ecclesiastics, however, and charitable institutions continued to funnel the energies and wealth of varied urban constituencies into charitable activity.

This is a slim book with a vast scholarly appa-

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ratus in the best French intellectual tradition. The substantial archival materials employed in the study did not, evidently, include demographic resources, so there is no way of knowing how many in the community of twenty-eight to thirty thousand took advantage of charitable services, nor the ties of those who did to the donors. The table of testamentary bequests suggests, nevertheless, that some broader questions could be addressed given the available data. Pious donors tended to give relatively equal bequests to five, ten, or even more charitable institutions rather than a single bequest. This helped maintain the autonomy of the various institutions and impeded the consolidation of services for the unfortunate into a few central urban facilities. Was this intended or fortuitous? What purposes were served by maintaining so many small charitable institutions? The relationship of eleemosynary activity to urban politics might yield very interesting perspectives. It is hoped that the author, who possesses a commendable control over the available source material, will employ it to answer some of these broader questions so that Narbonne may be viewed in comparative perspective with other Mediterranean communities.

SUSAN MOSHER STUARD
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ALAN E. BERNSTEIN. *Pierre d'Ailly and the Blanchard Affair: University and Chancellor of Paris at the Beginning of the Great Schism.* (Studies in Medieval and Reformation Thought, number 24.) Leiden: E. J. Brill. 1978. Pp. x, 307. f 120.

This scholarly work deals exhaustively with the ouster of John Blanchard, chancellor of the University of Paris, by that faculty, 1384–86, an ouster spearheaded by Pierre d'Ailly. Both sides drew up depositions, the papal delegate collected the testimony of eighty witnesses, Parlement kept transcripts, and d'Ailly wrote lengthy treatises, two of which Alan E. Bernstein has edited from manuscripts and included. In an eighty-three-article indictment the chancellor was accused of systematizing the fee exactions for the license to teach, of corrupt administration of a university privilege, of extorting money or goods from graduate theological students, of ranking theological students without faculty consultation, and of forcing bachelors by threats and promises to abandon their masters and study under him.

Blanchard claimed that his accusers were moved by spite or were angry as faculty members that he had inadvertently taken the rector's top-spot at a banquet. He did not deny that he had received money or ranked candidates but affirmed that these were prerogatives of his office granted by custom or papal letter. The attack of d'Ailly first concentrated on the exaction of oaths and gifts, stressing the sin of greed, then concluded that avarice leads to errors in faith and so accused Blanchard of heresy because the chancellor maintained that illicit practices are licit. All these matters are presented in great detail as the author carefully quotes from d'Ailly's speech "Radix Omnium," delivered at the College of Navarre in "questio" format, and from a second speech, "Super Omnia," in disputation form. Throughout his book the author has delicately weighed accusations, charted the frequency of the alleged bribes, and pointed out the strengths and weaknesses of the various arguments and rhetorical devices.

In a concluding chapter the author briefly summarizes his book and then proposes some thought-provoking suggestions about the connections between this affair and later developments in d'Ailly's theories about the Great Schism and conciliarism. The two speeches in the original Latin form the appendix. It is unfortunate that translations could not be given, but preceding chapters give paraphrases of some parts of them.

There is much of interest here besides the "Blanchard Affair" for readers interested in medieval and Renaissance history. For example, the first chapter is a fine summary, based both on older and on recent books, of the administrative development of the University of Paris and the quarrels that deeply affected that development. It is not easy for a modern academician to understand them or their ramifications; but this chapter, buttressed with many documentary quotes, certainly helps. To this reader it seems strange that modern discussions about the quarrels of mendicants and secular masters seem often to stress William of St. Amour's writings but do not show much interest in the answers of Aquinas, also a Paris professor, whose work on this matter has not only been published but translated.

All in all the title of this important book does not measure up to all the good things that are contained in it, which is somewhat of a phenomenon these days.

LOWRIE J. DALY
Saint Louis University

ARLETTE HIGOUNET-NADAL. *Périgueux aux XIV^e^ et XV^e^ siècles: Étude de démographie historique.* (Études et Documents d'Aquitaine, number 4.) Bordeaux: Fédération Historique du Sud-Ouest. 1978. Pp. 458.

Both in its method and its conclusions, this study is a significant contribution to medieval demography. As in much medieval social history, Arlette Higounet-Nadal's method is determined by the nature

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and extent of available documentation. The principal sources for the demographic history of Périgueux are fiscal documents: the French royal hearth census of 1328 and a series of tax rolls from the period between 1330 and 1493. The author is candid about the limitations of using fiscal records for demographic history; she acknowledges that estimating total population is far more complex than applying some arbitrary multiplier to the number of taxable households. Tax rolls tend to ignore whole sectors of the population, notably those below a certain threshold of wealth, women, and the exempted nobility and clergy. The author shows that the town did not need to tax every household all of the time and that households within a family group took turns paying the tax. Because of the uncertain relationship between fiscal population and real population, the author stresses that her figures lack statistical precision and should be seen only as minima.

In the face of these difficulties in estimating the population of Périgueux, Higounet-Nadal devised another method, which involved an exhaustive analysis—unassisted by the computer—of *all* the archival material concerning Périgueux in the fourteenth and fifteenth centuries. It is perhaps here that the originality of her work lies, for this method enabled her to suggest more about medieval populations than the quantitative measure she sought. For example, the section on topography offers an important challenge to the cherished notion of a direct relationship between demographic oscillations and the fluctuations of the urban area enclosed by walls. In Périgueux, the area remained constant while the density of occupation varied.

Higounet-Nadal's major conclusions both confirm and question received ideas on the dynamics of medieval population. There were, first of all, great variations in the town's total population, and the oscillations were more numerous and closer together than would appear from other studies. The book reopens the question of population levels in the early fourteenth century, confirming contemporary estimates—long mistrusted by historians—of very high levels. The author helps re-evaluate the role of the Black Death as the great exterminator of Europe's inhabitants. Figures for Périgueux confirm the now spreading idea of a demographic recession before 1348; it is perhaps the only town whose records show just how brutal this decline was—its population diminished by half even before 1348. An important conclusion concerns the extreme mobility of late medieval urban populations. The life span of families was short: in Périgueux 95 percent lasted less than two generations. Mobility was particularly characteristic of the less prosperous artisans and *laboureurs*. Urban population thus was perpetually renewed by immigration. Although statistics are impossible, general migratory patterns emerge clearly; most immigrants came from within forty kilometers, but some came from distant regions that functioned as population "reservoirs," for example, Brittany and the Auvergne.

These conclusions confirm and advance the work of medieval demographers. Carefully comparing her own findings with data from other towns and regions, the author illuminates medieval population movements well beyond the walls of Périgueux.

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Yale University

MODERN EUROPE

ELIZABETH L. EISENSTEIN. *The Printing Press as an Agent of Change: Communications and Cultural Transformations in Early-Modern Europe.* In two volumes. New York: Cambridge University Press. 1979. Pp. xxi, 450; 453–794. $49.50 the set.

No one has thought more intensely and imaginatively about the larger historical significance of the transition from script to print than Elizabeth L. Eisenstein; and the result, after a series of stimulating articles, is this long and important book. It is focused on the Renaissance, the Protestant Reformation, and the scientific revolution; and it argues that these movements, long conventionally identified with the origins of modern culture, were variously facilitated, shaped, spread, and perpetuated (i.e., given permanent effect) by the printing press. This remarkable piece of technology is thus, for Eisenstein, not simply one item in the list of novelties marking the transition from the medieval to the modern world; it is sui generis and ubiquitous, crucially collaborating with and conditioning everything else. The development of the printing press thus emerges here as the one truly revolutionary event in that great transition.

Since the Renaissance was well under way before the coming of print, this argument requires the division of the Renaissance into two phases: an earlier phase before the printing press, seen here as differing little from the classical revivals of the Middle Ages and without originality, which would have been as transient as those "renascences" but for print; and, after its coming, the "more expansive and intellectually comprehensive," but above all permanent, Renaissance. This notion seems rather forced, but Eisenstein's argument in connection with the Reformation is simpler, since the importance of printing for the spread of Protestantism has been widely recognized. She finds, however, much more to say than this. Since printing had prepared the way for Luther by shifting the control of pub-

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lication from clergy to laity, by making Bibles available, and by hinting that the burden of religious communication might be transferred from pulpit to book, and since it then gave his movement a success denied to earlier heresies, she argues that a history of the Reformation should properly begin with the printing press. Print, furthermore, facilitated some of the most celebrated results of the Reformation; for example, the availability of printed vernacular Bibles helped Protestantism to shape popular religious sensibilities and contributed to the formation of national cultures. But, in addition, some of the more important developments commonly attributed to Protestantism, Eisenstein maintains, in fact owed at least as much to printing. Thus, Weberian rationalization found a uniquely important model and impetus in the organization of the printing industry, and again printers prepared the way for the Enlightenment by the publication of subversive books. She uses much the same strategy in dealing with the rise of modern science. Once more the printing press prepared the way for a movement otherwise inexplicable by making available for the reflection of scientists what had previously passed for scientific knowledge. Later, it not only diffused the results of scientific activity but also continued to show scientists where they stood in relation to a cumulative tradition of scientific advance and, therefore, what problems next required attention; it bridged the gap between academic theory and artisan practice; and it brought a previously esoteric science into contact with political and religious life. This very selective account of Eisenstein's argument hardly does justice to its resonance. She manages to relate almost every development of interest to historians to the "communications revolution" effected by print.

It is a pity, therefore, since this is a book that every historian of early modern Europe should ponder, that Eisenstein so often invites the resistance even of an initially receptive reader. Her tone is that of polemic or debate rather than of sober reflection. She is not content merely to state her case, which is often powerful and always provocative; she also insists on attacking a host of other scholars, some long dead, not only for sins of commission in venturing views contrary to her own, but also for sins of omission in neglecting the importance of print. Among her targets—their views not always fully digested—are Panofsky, Seznec, Baron, Kristeller, Hexter, Gay, Thorndike, Zilsel, Weber, and T. S. Kuhn; and her boldness is especially remarkable since her own strong views are based chiefly on secondary literature. Her acquaintance with recent scholarship is impressively broad; but this hardly provides a sound base from which to launch an attack on, for example, the notion that the humanists of the earlier Italian Renaissance developed some sense of historical perspective. One ought to be able to make a case for the significance of printing without minimizing the importance of everything else.

This problem points in turn to another: that the book is, finally, only half a work of history. For although it is concerned with the broad and complex historical consequences of print, it tends to take the printing press itself entirely out of history. For Eisenstein, print seems to be an independent force in human affairs, a cause but never a result of historical processes. She refuses to see it as a tool, brought into existence and exploited by forces outside itself. That it sometimes became more than a tool is plausible enough; and the printing press may be important, among other reasons, as an early instance of the fact that, in the modern world, man may himself be shaped and victimized by his tools. But it still seems to me that we must begin by treating printing as a product of human ingenuity responding to historical pressures.

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FRITZ K. RINGER *Education and Society in Modern Europe.* Bloomington: Indiana University Press. 1979. Pp. 370. $22.50.

In this book Fritz K. Ringer argues that in several European countries the institutions of secondary and higher education initially injected into society incongruent elements of status and tradition but generally came to be functionally integrated into modern industrial life. They now have lost their autonomy and have become mere adjuncts of occupational structure. Ringer fleshes out his thesis with descriptions of schools and universities in Germany, France, and England, as well as, to a lesser extent, in the United States. He relies on historical enrollment data and on correlations of the social status and the occupations of students and their fathers. He distinguishes an early industrial phase before the 1860s, in which there was hardly any connection between higher education and the economy, from a high industrial phase, when through scientific, technical, and applied studies education began to contribute directly to economic growth, and from a late industrial phase beginning in the 1930s when education and the economy began to converge. Ringer shows that during this process the proportion of the overall population attending schools increased but did not bring in an increased share of the lower-middle or lower classes. Thus, while education everywhere became more inclusive, it did not necessarily become more progressive.

As Ringer interprets his data, they show that before the 1860s Germans held on to a preindustrial

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conception of higher education as a status-conferring preparation for professional and governmental service. During the high industrial phase the influx of the sons of the propertied bourgeoisie led to the appearance of nonclassical secondary schools and technical universities, now providing an alternative pathway for a different social group. In France, by contrast, the sons of the wealthy bourgeoisie attended the classical *lycées* and *collèges*, and later attended the *grandes écoles*. Among these, only the *École Normale* resembled the German universities in its emphasis on training academics. Ringer sums it up thus: "In France, the antithesis was between the academic and the integral bourgeois; in Germany, it opposed humanistic cultivation to the world of practicality, commerce, and technology" (p. 205). In England, Ringer finds a fairly uniform generalist curriculum from the colleges at Oxbridge to the civic universities and various secondary schools, instruction quite unresponsive to the practical demands of industry, yet combined with a very progressive social system. The few pages Ringer devotes to higher education in the United States are marred by questionable generalizations. Ringer treats eighteenth- and nineteenth-century colleges as cut from the same cloth, and he allows no room for differences among the nineteenth-century institutions.

Ringer believes that our chief problems today derive from disappointed expectations. Greater inclusiveness has devalued education in the eyes of middle-class students, who no longer anticipate higher status, a more affluent life-style, and professional independence but must look forward to joining the white-collar army. In Ringer's view, however, this devaluation only lessens education's social, not its intellectual, benefits, and when it is coupled with meritocratic principles it ensures greater progressiveness. Add to it reduced segmentation (tracking), and increased inclusiveness will avoid "a dangerous concentration of power" in the hands of a few (p. 267). It will extend common schooling upwards, and the advanced schools will cease thereby to legitimatize and perpetuate society's class structure. Ringer, however, sees no way in which we could prevent the loss of education's humanistic tradition and partial autonomy. Such, we are led to conclude, is the price we pay for social and technological progress.

Essentially, this book is an extended argument derived from the nearly one hundred pages of appended statistics. It is meant to serve, writes Ringer, as a temporary foundation for the "young field" of the comparative social history of national systems of secondary and higher education (p. 1). The statistics as interpreted by Ringer certainly back up his thesis. This reader, however, wondered what other theses these data might support once another historian asks different questions. In short, this may indeed be a very temporary foundation, and as the field grows older the insights and conclusions are likely to become far more complex than they are made to appear in this volume.

JURGEN HERBST
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YUKIO TOMINAGA *et al. Fuashizumu to Kominterun* [Fascism and the Comintern]. Tokyo: University of Tokyo Press. 1978. Pp. xi, 359, 45. 1,800 yen.

The publication of this book was welcomed in Japanese historical circles. The authors, specialists in modern German history, share a common Japanese abhorrence of fascism and an apprehension that it might reemerge, but these sentiments do not pervade their study of the Comintern's analysis of fascism. Unlike many Japanese studies dealing with fascism, the authors' approach is not weakened by rigid adherence to an ideological framework leading to oversimplification.

The authors state in their introduction that their objective is to provide a history of the Comintern's theory of fascism, their emphasis being on "history," which they say has been lacking in previous Japanese treatments of the subject. Their approach, however, seems closer to a Chinese than a contemporary Western understanding of writing history, being framed in the mode of a descriptive history of ideas rather than an analysis of the development of ideas in their social, political, or institutional context. Using a wide range of official Comintern publications, writings by individual Comintern leaders, and Western secondary sources, the book thoroughly outlines the numerous views of fascism presented in the Comintern from the early 1920s until the 1935 Seventh World Congress. In doing so, it gives a picture of greater theoretical diversity and vigor than many studies of Comintern theory, which tend to focus primarily on the resolutions and theses representing the Comintern's official policy. Comintern leaders differed most in their opinions on the nature of fascism during the 1920s, but even after the bolshevization of Communist parties and the Sixth World Congress's adoption of "social fascism" to define social democracy, the concept did not find immediate acceptance among all branches of the Comintern.

The book does not attempt to show the sources of such diversity by relating the various views to concrete historical conditions. For example, there is virtually no analysis of the effects of internal Soviet political conditions on Comintern views. The authors' specialization is evident in their more extensive examination of German Communist Party positions, but even here references to political events

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are not integrated into an explanation of party actions and decisions.

As a secondary study, the authors therefore provide a more complete and detailed treatment of the variations of opinion within the Comintern than has previously been available in any language. For specialists in the field, however, their reliance on published sources means that they do not present any surprising information. The absence of interpretation consequently makes the book of less interest to Western historians than Japanese.

ELISE K. TIPTON
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MARTIN MCCAULEY, editor. *Communist Power in Europe, 1944–1949*. New York: Barnes and Noble, for the School of Slavonic and East European Studies, University of London. 1977. Pp. xxiii, 242, $21.50.

The thirteen chapters in this compact volume were originally presented at a series of seminars held in 1974–76 at the University of London's School of Slavonic Studies. The authors were concerned to take another look, this time from the perspective of thirty years, at the European aspect of the great Communist surge that dominated the history of the late 1940s.

The conclusions that emerge from these chapters are not startling but still very much worth restatement today. In Europe, Communists everywhere constituted a minority—often assiduous—of the electorate. Even in those Soviet-occupied countries (East Germany, Czechoslovakia, Hungary) where the Communists at first believed they could compete at the hustings, they had to face bitter disappointment. Only where the Soviet army was present, or close by, was Communist power possible. In many countries, Poland especially, the immediate problem was to find enough cadres to operate the government on a day-to-day basis, this being one reason for the forcible absorption of the socialist parties. Hugh Seton-Watson argues with some eloquence that the true origins of the Cold War are not to be found in any feature of American foreign policy in 1944–49 but instead in Western reaction to the treatment that the Communists had to mete out to their new subjects simply in order to maintain control.

It was still true, the thirteen chapters make clear, that in 1944–49 a British or American military presence was necessary to save Greece, Italy, and France—countries where communism was espoused by as much as a fourth of the electorate—from Communist rule.

In this work there are new insights also. Even in Poland there existed a small core of dedicated Communist idealists whose presence was essential to the functioning of the system. The true significance of the brief Soviet occupation of Czechoslovakia was that it endowed the Communists with a monopoly of local government in that country. Togliatti in Italy abandoned the revolutionary cadre party, seeking to block the return of Fascism by creating a mass Communist Party, a shift in strategy that made difficult the constraint of militants aching to start a civil war that they could not win.

There are also matters to be criticized. The absence of a chapter on communism in Yugoslavia constitutes a sizable lacuna, one which contributes to underestimation of the role of the partisans and, as well, of the Slavo-Macedonians in the Greek civil war. Nor can I agree that the evidence for a decline in Czech voting strength after 1946 is doubtful, or that it was much easier for Gottwald to take power by a February coup than by a single-list election in June 1948.

Nonetheless, *Communist Power in Europe* is a useful book, a full cut above the average multi-authored work, a helpful exercise in historical perspective, a good buy for even the small town library.

R. V. BURKS
Wayne State University

PATRICK O'BRIEN and CAGLAR KEYDER. *Economic Growth in Britain and France 1780–1914: Two Paths to the Twentieth Century*. Boston: George Allen and Unwin. 1978. Pp. 205. $21.25.

In the late 1940s and in the 1950s, it was axiomatic that the nineteenth-century French economy was retarded by comparison with Great Britain. The task of economic historians was to explain why the French lagged. In the 1960s, the tenets of the retardation school were subjected to increasing critical scrutiny, and, by the 1970s, the myth of French economic retardation had been dispelled. The record of France's economic performance had been so refurbished that François Crouzet's characterization of it as "certainly not brilliant, but quite creditable" could command general assent. On the basis of their exercises in quantitative history, Patrick O'Brien and Caglar Keyder have buttressed this view with a detailed comparison of the two countries.

They have constructed decennial estimates of domestic per capita commodity output for Britain and France over the period 1781–1913, which show that France was ahead on the eve of the revolution, fell behind during the revolution, but forged ahead by midcentury, lost the lead again for the two decades 1885–1904, but regained it in the years before the First World War. Except for the period of the revo-

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lution and Napoleonic wars, the levels were never far apart. Services, in which Britain possessed a decided advantage owing to its large foreign trade, are omitted for lack of adequate data. The inclusion of services would give the British an advantage over the French that the authors guess might amount to about 15 percent, a figure they regard as too small to warrant any adverse reflection upon French economic performance. They have also provided a comparison of labor productivity between the two countries that shows Britain maintaining a modest lead between the revolution and the First World War. Although productivity in French agriculture was well below that of Britain, France, surprisingly, led Britain in industrial productivity until the 1890s. Since statistics are "merely a prelude to historical investigation," the authors provide a detailed analysis of such questions as: Why did French agriculture lag? Finally, they argue that it is an error to regard the British model as the norm for industrialization; the French managed to industrialize by a different route, a route the authors see as less cataclysmic and involving fewer social dislocations.

Acceptance of their conclusions depends in large part upon how much confidence may be placed in their quantitative data. For the most part, their calculations are based upon the standard quantitative works for the two countries, especially those of Phyllis Deane, W. A. Cole, and B. R. Mitchell, for Britain, and Jean Marczewski, J. C. Toutain, and T. J. Markovitch for France. It is beyond the scope of this review to pass judgment on these sources, except to observe that they do raise some questions. For example, although O'Brien and Keyder recognize that Toutain's estimates of French agricultural output for the eighteenth century have been much criticized, they use them all the same. Certainly, the comparisons of industrial and agricultural productivities for the two countries for the decade 1781–90 on page ninety-one are highly questionable. Still, subject to some caution, their quantitative exercises must be taken seriously.

The opening chapter on the historiography of the retardation thesis omits any discussion of the critics of the thesis, which gives the authors' conclusions an appearance of greater originality than they deserve. From their discussion, an unwary reader might assume that Rondo Cameron was one of the proponents of the retardation thesis. Today, anyone arguing against the retardation thesis must be regarded not as challenging accepted views but in the mainstream. These observations are not meant to deny the substantial contribution of O'Brien and Keyder, only to put it in proper perspective.

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ALEKSANDRA HELENA KASZNIK. *Między Francją a Algierią: Z dziejów emigracji polskiej, 1832–1856* [Between France and Algeria: From the History of the Polish Emigration, 1832–56]. Summary in French. (Polska Akademia Nauk—Oddział w Krakowie, Prace Komisji Nauk Historycznych, number 40.) Wrocław: Ossolineum. 1977. Pp. 171. 40 Zł.

This monograph is an abbreviated version of the author's doctoral dissertation. It focuses on Franco-Polish relations, 1832–56, as affected by the presence of Polish refugees in North Africa. The monograph is divided into two almost equal parts. The first deals with civilian Polish refugee-colonists; the second, with those Polish émigrés who served in the newly formed French Foreign Legion in Algeria. Each part is meticulously subdivided into nine chapters varying in length. In each chapter the author skillfully presents and judiciously analyzes the material. The work is based on extensive French and Polish archival materials, supplemented by references to contemporary French, Algerian, and Polish émigré newspapers. It lacks both index and bibliography, but maps and a useful French summary are included.

Aleksandra Helena Kasznik convincingly argues that, between 1832 and 1856, Polish refugees in Algeria were a significant factor for official French policy makers. French national interests in Europe and North Africa conveniently coincided with the rather unsettling presence of large numbers of Polish émigrés in France and additional émigrés requesting permission to settle there. The French government, especially after 1832, pressured a significant number of reluctant Poles to emigrate to North Africa. Polish civilians acquiesced and established pioneer settlements in the newly acquired French colony of Algeria. Polish military émigrés, diverted by the French government, served in the newly created Foreign Legion, protecting French interests.

Official French policies frequently ran counter to the proposals of Polish émigré leaders. But the Poles, given few options, usually acceded to French demands. This point is made repeatedly throughout the text. It is especially well taken when the author focuses on the Algerian project of Adam Czartoryski's Paris-based Hotel Lambert, the major Polish émigré group in direct contact with the French government. Carefully analyzing Czartoryski's project over a twenty-year period, Kasznik emphasizes the basic significance for it of those Poles serving in the French Foreign Legion. They were to serve as the nucleus of an army to be created in Europe to fight, at the appropriate time, for Polish independence. Despite Czartoryski's persistent efforts to achieve this objective, the French government repeatedly

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refused to permit the Algerian Poles to participate, in any manner, in the European crises of 1846 and 1848 or in the Crimean War. After 1856, few Poles emigrated to Algeria. Their numbers there, and in the Foreign Legion, declined to insignificant proportions. As a result, the Algerian Poles no longer played an important role in French policies or in abortive Polish émigré projects.

Kasznik's monograph is informative about the civilian and military life of Polish émigrés in Algeria and the role these refugees played in Franco-Polish relations. Her disciplined and skillful analysis of archival materials and her succinct presentation result in an admirable contribution to the history of the Polish emigration as well as to the history of French colonial interests in North Africa.

JOAN S. SKURNOWICZ
Loras College

JOAN THIRSK. *Economic Policy and Projects: The Development of a Consumer Society in Early Modern England.* Oxford: Clarendon Press. 1978. Pp. vi, 119. $14.95.

In this enthralling book, based on her Ford Lectures at Oxford in 1975, Joan Thirsk returns to a theme that she first explored in her well-known essay on "Industries in the Countryside" in the *festschrift* for R. H. Tawney in 1961. "Projects" are a familiar, indeed notorious, topic to historians of the early modern period, and the author does not discount the monopolistic abuses to which many of them ultimately gave rise. But she also distinguishes between the "constructive" and "scandalous" phases in their history and argues persuasively that even in the latter, after 1580, there was "a strong tide of healthy economic energy" (p. 52). It was this that encouraged the development of many of the new industries of the time and gave rise to widespread new employment. It is one of the merits of the book that it envisages the period of "projects" not as a mere political or commercial interlude but as symptomatic of a fundamental change in the English economy.

The basic thesis of Thirsk's essay of 1961 was the appearance of by-employments in many pastoral areas, as population doubled in the Elizabethan and early Stuart eras. This thesis has been taken up by many subsequent scholars and amply demonstrated by further local and regional research. In the present volume, which ranges more widely and is not wholly confined to rural industries, the author adduces substantial evidence for the evolution of more than a hundred new occupations. Amongst them, for example, were stocking knitting, lace making, nail making, soap boiling, tool making, linen weaving, pin making, paper making, distilling, and thread manufacture, some of which, such as lace making and linen weaving, certainly employed many thousands of provincial people. Individually, industries like these may not seem epoch-making; but many of them served basic human requirements, and taken together they effected a gradual but far-reaching revolution in the economy. The traditional concern of economic historians with a limited range of staple industries, such as clothing and coal mining, can thus be seen in a new light. Important though these were, they clearly formed no more than part of a highly complex web of novel and expanding trades.

Thirsk's book is thus more far-ranging in its implications than its modest length and somewhat unalluring title might lead us to suppose. It is vivid, moreover, with something of the same warmth of humanity as the writings of R. H. Tawney himself. That is a quality conspicuously absent from the work of many economic historians today, and its absence has not only made a journal like the *Economic History Review* intolerably dull but also impoverished our understanding of history itself. This is a book, in short, that searches deeply into the "dark corners of the land," introducing us to many humble occupations, humble individuals, and humble communities—particularly in the pastoral areas of the country, but also in provincial towns like Winchcombe and Maidstone and in the back streets of London itself. Like Thirsk's earlier work it will surely give rise to much further research, for it is in such lowly origins as these that we see a relatively poor and backward country beginning to transform itself into the first industrial nation.

ALAN EVERITT
University of Leicester

BERNARD J. VERKAMP. *The Indifferent Mean: Adiaphorism in the English Reformation to 1554.* (Studies in the Reformation, number 1.) Athens: Ohio University Press, and Wayne State University Press, Detroit. 1977. Pp. xviii, 201. $13.00.

This is a most stimulating and learned essay, which no student or scholar engaged in Reformation studies ought to miss. Yet is seems to me to be based on a misunderstanding that will not, I hope, be perpetuated among Reformation addicts, who are always attracted in these day by an ideological rather than a theological rationale that will safely cramp the Reformers within the "history of ideas." For, in fact, it may be suggested that there was no "Adiaphoristic theory" or adiaphoristic thrust" (ugh!) or "main line adiaphorists," and the Reformers did not accost one another with "Ah! I see you are an adiaphorist!" There are a number of issues in which the distinction between things essential and things "indifferent" are involved. There is the classical and

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philosophic conception of the golden mean and, not unconnected with it, the thought of "epieikia"—that equity which adjusts strict justice to circumstances. There is the important New Testament discussion, especially in St. Paul, about things that affect Christian liberty, the concern for the "weaker brother" and, implicity, the repudiation of any Christian Torah, and the question of the validity of the moral and ceremonial law. Some clear differentiation of these elements in the New Testament would have eased the later discussions in this book. When we come to the late medieval and early Reformation period, the complaint against a Christianity overburdened with laws and legalism, as vehement in Gerson and Colet as in Wycliffe, and the humanist concern for a return to the charity of the original Christian gospel prompt a distinction between the essence and the nonessentials of the gospel. Bernard J. Verkamp pays learned attention to the Continental Reformers (indeed gets on rather better than with the English ones), but he misses some important clues in Luther: namely, Luther's fundamental distinction in his "Babylonish Captivity" between the scriptural essentials of the gospel, which he lists, and the traditional nonessentials that he also records. Luther elaborates this distinction in his Confession of Faith (1528), which, as Maurer has shown, was taken up by Melanchthon in the Augsburg Confession. For Melanchthon the distinction between the gospel itself, about which there could be no compromise, and the so-called abuses was all important. It is a pity that there is no coherent discussion of Melanchthon and that the great Interim debate, which really does concern "adiaphora," is reduced to a long footnote and a couple of paragraphs. The difference between Melanchthon and Bucer at this point is important and not irrelevant to the English scene, especially in the reign of Edward VI. There is the further question of how far the authority of the Bible and the godly magistrate are of determinative value in making things indifferent. There is the stress on diversity within unity that permitted as much liturgical and ritualistic variation. And, finally, there is such pressure on the distinction between essentials and secondary matters as leads, on the other hand, to a premonition of the doctrine of toleration (as the author rightly sees it in Frith and Tyndale) and, on the other hand, to the establishment of Anglicanism as a *via media*. It is a pity the author did not go on to 1560 and include Parker, who had really more important things to say than Starkey, who is overrated in importance in this volume.

I do not think the author really grasps why Robert Barnes got into such prolonged trouble over his affirmation that human laws do not bind conscience. It really looks as though he took over Luther's affirmation to this effect in his "Babylonish Captivity," where it was picked on by More and Fisher. Despite his modification of this in the the second edition of his "Supplication," it was too good a weapon for his enemies to omit. More importantly, and well worth discussing, is the sentence thrown out (or falsely wrung out) of John Lambert to the effect that the magistrate has no authority to make compulsory what is really indifferent, although this is an issue that comes out more clearly in the famous set-to between Hooper and Ridley. With regard to these half-dozen related but really very different issues, the author produces a constant stream of learned and perceptive comments, and his judgments seem to me always sane and sound (that is, in his just criticism of Coolidge). At times he hops about from one scene and period to another: it does not seem to me to be very helpful to compare Frith's view about the eucharist with Flaccius's views on the Leipzig Interim. I hope in future we shall hear less rather than more about "adiaphorism" and that more consideration may be given to the half-dozen important problems here squeezed into one. They really do not make one package deal, and the differences between some of these distinctions are more important than the similarities. In any case one may bespeak many readers for this stimulating and sometimes excitingly learned work.

GORDON RUPP
Cambridge University

MARJORIE BLATCHER. *The Court of King's Bench, 1450–1550: A Study in Self-help.* (University of London Legal Series, number 12.) London: Athlone Press, University of London; distributed by Humanities Press, Atlantic Highlands, N.J. 1978. Pp. xiv, 181. $23.00.

Most historians of English law have dealt with the revival of the common-law courts in the fifteenth and early sixteenth centuries in terms of law reform or of the response of common lawyers to a threatened reception in England of a renascent Roman law. Marjorie Blatcher has placed her excellent study in the context of the debate surrounding the reception question with particular attention to Maitland's and Holdsworth's contributions. While Maitland was convinced that the reception was narrowly prevented only by the resistance of the common lawyers, Holdsworth held that the reception had been a matter of little consequence to English lawyers. Blatcher is not content with Maitland's conclusions nor is she satisfied with Holdsworth's methodology. Thus, she has prepared a measure with which to test their conclusions: "the return of the profits of the seals of the two benches," a reference to the crown side and the plea side of King's Bench. Her results are gratifying.

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Medieval government lacked confidence in the ability of the Court of King's Bench to deal with matters of public concern because the court was unable to force defendants to appear and answer its process. Because the pains of outlawry and distraint held no terror, the court lacked effective enforcement mechanisms. These shortcomings have led Blatcher to ask a fundamental question: "Was it really worth anyone's while to sue in a common law court when the odds were so heavily against getting the defendant into court or obtaining a decision if one got him there?"

The sheriffs were responsible for the enforcement of process and their record was abysmally poor. Finding that the system was in shambles, Blatcher concluded that the circumstances were worsened by irresponsible general and special pardons after the late fifteenth century. In one sample period, Michaelmas Term 1488, thirty pardons were pleaded for offenses that had been brought before the court, on the average, eleven years before. In the meantime, the defendants had escaped the consequences of crimes as serious as murder and treason. Blatcher believes that the clerks allowed these delays to occur, and for a price.

Reform was needed, but the only external force capable of instituting reform was the king. He, however, had a vested interest in preserving the status quo. If King's Bench proved unsatisfactory to him, he had at his disposal the new conciliar courts such as Star Chamber, the Court of Requests, and the regional councils. Their existence gave the king a good excuse to do nothing about the problems of the King's Bench. Furthermore, the common-law courts were populated by myopic lawyers who feared that any overt acts that they might make would draw attention to themselves. In the face of loss of business and an ingrained defensiveness, they hid their heads and ignored the problems that faced them. Collectively, the common lawyers could have mounted a formidable force for change, but their inability to act in concert was nearly catastrophic.

A remedy did emerge from the procedure of the court and from its bench. The Bill of Middlesex was a patent fiction that was used to establish a national competence for the King's Bench writs, while the writ of *latitat* permitted the physical arrest of a defendant not actually in Middlesex. The process of change had begun in 1452 when the Bill of Middlesex was introduced, and the transformation was completed by 1542 when it became unnecessary to allege the bill; *latitat* alone became a sufficient first process rather than an enforcement measure. "Defendants who had long laughed at outlawry were to suffer genuine hardships from the arbitrary use of the *latitat*."

The literature of legal history has been overcomplicated by the reception question. Blatcher has convincingly and fairly disposed of it. The questions of reception and of legal reform were simply inappropriate methods of discovering the cause of the revival of the central common-law courts in the sixteenth century. Answers have traditionally been sought in cultural and intellectual contexts, but Blatcher has patiently uncovered the answers where they have been all along: in a careful reconstruction of the procedure of the court of King's Bench.

There are two critical points that must be mentioned. First, a glossary would have assisted all but the most technically sophisticated reader. Second, the case can be made for conceiving of "common law" not only in the narrow sense of law by precedent as practiced in the common-law courts, but also in the broader sense of that body of law and practice common to all Englishmen. Although this interpretation would not seriously alter Blatcher's conclusions, it would assist us to understand better the behavior of the competitors for the custom that the King's Bench was determined to monopolize.

LAMAR M. HILL
University of California, Irvine

CHARLES CARLTON. *The Court of Orphans*. Leicester: The University Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1974. Pp. 145. $10.50.

Charles Carlton's slim monograph, based on extensive research in the city archives of London, Bristol, and Exeter, is doubly welcome. On the one hand, it offers a wealth of information to legal historians about a little-known aspect of early English inheritance law and the courts that administered it. On the other, it deals with a problem central to the lives of persons of "the middling sort," whom historians so often neglect in favor of the aristocracy or the poor: how to protect their children's future in a time when one child out of three could expect to lose a parent before attaining adulthood.

For four centuries the court of orphans provided a solution to the problem. Consisting of the mayor and aldermen, it played—and played effectively—a fourfold role. As a department of municipal government, it was an administrative body. In its supervision of the estates of freemen's orphans in accordance with customary law, it exercised limited judicial functions. As custodian of orphans' inheritances, it became an important financial institution that supplied both private credit and municipal finance. And by standing *in loco parentis* to orphans, arranging their marriages, preserving their capital, and ensuring the transmission of wealth from generation to generation, it served the small part of English society over which it had jurisdiction—the freemen of the boroughs—as a mechanism for preserving social stability.

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Carlton not only analyzes the multiple functions of the court but also traces its career chronologically from its medieval origins in the thirteenth century, through its years of expansion in the late sixteenth and early seventeenth centuries, to its demise, for all practical purposes, with the passage of the Orphans' Act in 1694. How the court arose from the needs of townsmen who had purchased freedom from feudal servitude at the price of subjection to the whims of trade and the insecurities of urban life; how it expanded in response to changing attitudes toward family and children, demographic changes, and local circumstances; and how it declined as its merchant clientele found new ways to pass on wealth to their children and as increasing centralization brought Chancery, with its nationwide powers of enforcement, into a position of dominance over local courts of equity, are all part of the story. It is one worth the telling, and Carlton tells it well in straightforward, lucid prose, enlivened with occasional bits of sly humor.

RUTH K. MCCLURE
Yale University

LEAH SINANOGLOU MARCUS. *Childhood and Cultural Despair: A Theme and Variations in Seventeenth-Century Literature*. Pittsburgh: Pittsburgh University Press. 1978. Pp. xii, 305. $11.95.

Leah Sinanoglou Marcus originally intended a purely literary study of the use of the child's image and *persona* by certain English poets. But it has broadened naturally into a theory of considerable interest and importance for cultural history. In general, she extends George Boas's thesis that the cult of childhood is a focus for cultural nostalgia in times of social upheaval. In the fourteenth century, as she shows, this was expressed by Franciscan writers, and in the nineteenth century by the Romantics. During her own period it was used by ideological Anglicans: "For seventeenth-century Puritans, their children were the best hope for a better England to come. For conservative Anglicans, childhood was a symbolic link with an idealized England gone by" (p. 43). The questioning spirit of that century brought a reaction toward an animistic view and an intuitional approach to the world. Beyond that her poets (Herbert, Herrick, Crashaw, Vaughn, Traherne, Marvell) may have been reacting against the demanding and sometimes unsympathetic attitudes of Puritans. She does not imply that the Anglican cult affected child rearing, as the growing interest of Puritans is assumed to have done.

Even as cultural history, the study is not as broad as it might have been. Marcus does advance our knowledge of the theological and literary antecedents of the cult of childhood. But she does not link her study with the puzzling subject of eighteenth-century sentimentalism, as she might have done by mentioning the reception of her poets in that period. She does not discuss the Cambridge Platonists' very similar perception of the child's nature and innate perfection (described in John Passmore's article in *Aspects of the Eighteenth Century* [1965]). Her interesting references to the Continental origins of Anglican Pelagianism are too brief to contribute much to our understanding of the growing theological debate over human nature. But in view of what is accomplished, it is no real criticism to say that the study might have been expanded.

The few weaknesses in her presentation of the various authors' rather different uses of the child's image and outlook occur in her treatment of Traherne and Marvell. The former could not have remembered the prewar Establishment. And Marvell's nostalgic vision of the child's Eden seems less Anglican than Arcadian. It is only with difficulty that she restrains herself from psychoanalyzing Crashaw and Marvell. But the subject does invite such an analysis, since the poets were not speaking of the children they saw but of their own imagined childhoods. None of these qualifications invalidate Marcus's contribution, which advances our knowledge of the cult of childhood in a period that had previously been neglected.

C. JOHN SOMMERVILLE
University of Florida

J. W. WILLIAMSON. *The Myth of the Conqueror: Prince Henry Stuart, A Study of 17th Century Personation*. New York: AMS Press. 1978. Pp. x, 219. $21.00.

J. W. Williamson interprets the life of Prince Henry Stuart as the conscious acting out of the role of Protestant hero and future conqueror of Catholicism, a part that had been assigned to him by militant Protestants from his birth. Henry had accepted his destiny so completely that when he died at the age of eighteen, he had "so thoroughly wedded his life to his myth that disentanglement of the two would forever be impossible" (p. 162). Williamson analyzes portraits of the prince and poems, plays, essays, and books written for and about him to describe the origins and development of the myth. The book is significant for its contributions to our understanding of propaganda techniques in early modern England and of the frustrations of the militants. Students of the second half of James's reign will recognize that once the death of Henry had eliminated the focus of the anti-Catholic writers, they turned to more open criticism of James's policies, causing new problems for his government.

Williamson skillfully describes the literary and

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artistic development of the myth of the conqueror. Authors and painters established Greek gods and heroes and military conquerors as models for their prince. They made the tournament and the battlefield his proper settings and left no doubt that the enemy was popery. By the time James I recognized that the militants had gained control of his son's image, there was little he could do. Williamson treats Chester's *Triumph in Honor of Her Prince* and Ben Jonson's *Oberon* as examples of the belated creation of a rival myth, making Henry a fertility symbol and emphasizing the necessity of his remaining within his realm. Henry demonstrated, however, by his love of tournaments, interest in the navy and foreign expeditions, refusal to take a Catholic bride, and harassment of recusants in Chester that the idea of the Protestant conqueror was the one he pursued. Williamson does not prove conclusively that Henry adopted that persona because of militant propaganda, but he makes it clear that the prince's actions corresponded to the myth. He has made a strong case for the significance of the myth, but the insistence that old allegations that Henry was poisoned should be reconsidered, not because there is new evidence or a new suspect but because Henry may have been about to try to convert the myth into reality, is unnecessary to his case. The literary analysis and the coherent picture of the development of the myth are original; the idea that there were those who would profit from the death of the prince is not.

JEAN A. SCOTT
Duke University

KEVIN SHARPE, editor. *Faction and Parliament: Essays on Early Stuart History*. Oxford: Clarendon Press. 1978. Pp. vi, 292. $22.00.

The reading of this little volume on early Stuart history may prove to be a traumatic experience for a number of complacent Stuartists. A compendium of eight brief monographs of about 12,000 words each by seven different authors, it is directed at stimulating some serious rethinking of conclusions about the relationship between crown and Parliament in the period 1603–29. Kevin Sharpe, the general editor, offers an excellent introduction concerned with parliamentary history in the specified period, in addition to a later essay on the Earl of Arundel's opposition to the Duke of Buckingham, 1618–28. R. C. Munden's article covers the reform movement and the development of distrust between James and Commons, 1603–04. G. L. Harriss discusses medieval economic and political doctrines as related to parliamentary debates on royal supply, 1610–29. D. Hirst speaks of the court and country parties and politics prior to 1629. S. L. Adams pinpoints foreign policy and the Parliaments of 1621 and 1624. J. N. Ball investigates John Eliot and Parliament, 1624–29, and C. Thompson closes the series with an essay on the leadership of the House of Commons in 1629. In each monograph the author outlines the generally accepted view of the problem within his area of competence; then, with the aid of new evidence and the reanalysis of older documentation, he proceeds to offer alternative, and usually more realistic, conclusions.

According to the editor's preface this work is an especial attempt to place the questions raised for study into a seventeenth-century context: to try to see and understand problems and their proposed solutions as they appeared to the proponents and to more clearly understand the friction between the two primary constitutional institutions that led to civil war and to the development of modern representative government. The division of labor into carefully delineated problem areas and the consequent precise focus on a single, clearly identified historical incident makes for both efficiency and emphasis in presentation. Clearly, in both thesis and method the authors attain their stated objectives. In so doing they have produced a volume, all too brief to be sure, that will lead to both elation and argument, for they challenge the interpretations of certain historians long accepted as authoritative.

During the past century the view in this country of the James I–Parliament problem has been generally dominated by S. R. Gardiner's politically oriented *History of England from the Accession of James I to the Outbreak of the Civil War* (1900–04). This was modified about a half century ago by Wallace Notestein's *The Winning of the Initiative by the House of Commons* (1925). In 1950 George L. Mosse brought political philosophy and ideological concept into fuller play with his *Struggle for Sovereignty in England*. And, unfortunately, along the way several historians added the image of James I as a "slobbering" idiot (as per M. M. Reese) to our bibliography. Without doubt our portrait of the early Stuart period is in need of major repairs and reorientation. Sharpe and his colleagues offer at least a beginning in that direction. Yet, they do not claim an altogether new view of the Stuart crisis. Much, of a scattered nature, has been done during the past two decades toward a revision of the history of that period—without any real impact on the old and comfortable views so acceptable this side of longitude 65° W.

Where there is so much good it is unpleasant to shadow it with minor caveats. Students will find the sentence structure of one of the articles too cumbersome for easy assimilation. The seven-page index of about 5550 topical entries, mostly given over to proper names, is not in keeping with the quality of

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the volume. In this book, however, the Oxford University publishing facility maintains the high degree of material and editorial craftsmanship for which it is famous.

PAUL L. HUGHES
University of Wisconsin, Whitewater

STEPHEN FOSTER. *Notes from the Caroline Underground: Alexander Leighton, the Puritan Triumvirate, and the Laudian Reaction to Nonconformity.* (Studies in British History and Culture, number 6.) Hamden, Conn.: Archon Books, for the Conference on British Studies and Wittenberg University. 1978. Pp. xviii, 125. $12.50.

This sixth volume in a series, published under the auspices of the Conference on British Studies, is a short book but not a slight or unimportant one. The sober subtitle, "Alexander Leighton, the Puritan Triumvirate, and the Laudian Reaction to Nonconformity," accurately conveys the general scope of the work, but the suggestive title with its Dostoevskian echoes is both a witty conceit and a reflection of a larger purpose: to use the trials of Leighton and later of Burton, Bastwick, and Prynne as a means by which to enter into the twilight world of popular lay Protestantism in England in the late 1620s and 1630s, a world half-hidden from and almost wholly misunderstood by the ecclesiastical authorities then and largely unexplored by historians today. Stephen Foster's fresh angle of vision comes from asking a question we should have asked long ago: why did Archbishop Laud and the other Privy Councillors make a Star Chamber case out of the publication in 1633 of *Histriomastix*, a massive, turgid, unreadable, and probably unread attack on stage plays, and sentence its author, a lawyer without significant office, connections, or following, to perpetual imprisonment?

Foster explains this startling overreaction in general by noting the authorities' growing awareness of the Puritan practice of private meetings and conferences, which were seen as conventicles and associated with radical separatism, and in particular by citing the trial three years earlier of Alexander Leighton, author of that much more radical and direct attack on prelacy, *Sions Plea*. In the light of the conspiracy theory adumbrated in Leighton's case and confirmed in Prynne's, the later trials of Burton, Bastwick, and Prynne a second time become readily explicable. Having demonstrated why such a disparate collection of individuals should have come to be linked together in a conspiracy, at least in the official mind, Foster then examines what little is known about the clandestine trade in unlicensed "seditious" books and concludes reasonably that it was a "hit-or-miss" trade "that mostly missed." Its major success was in frightening a regime already prepared to believe that a vast conspiracy was afoot, aimed at the overthrow of church and state. ("The truth is, Mr. Prynne would have a new church, a new government, a new king," so Cottington charged.) What followed was a policy of repression that forced moderate Puritans, surely the vast majority, to choose between the national church and "an increasingly militant Puritan underground of gathered conventicles" (p. 67). The consequence for many was the radicalization of both thought and action: sedition and revolution became to a degree a self-fulfilling prophecy.

Foster is surely right in picturing Laud not simply as the arrogant strong man planning counterrevolution but rather as the frightened bureaucrat seeking to defend an apparently fast eroding position. What Foster may do less than justice to is the world as it must have appeared from Lambeth Palace. In the months between the publication of *Sions Plea* and Leighton's sentencing, Winthrop's party set sail for Massachusetts, Thomas Hooker's suspension provoked a petition in his behalf signed by forty-nine Essex clergymen, and Laud's future chaplain uncovered the existence of the Feoffees for Impropriations, to note only the most dramatic events of that time. When each dispatch brought fresh news of independent initiative, disaffection, and outright opposition, it must have been all too easy to see Leighton and the triumvirate as the propaganda arm of a vast conspiracy, all the more formidable because its dimensions, as Foster shows, were only partly glimpsed.

PAUL SEAVER
Stanford University

DONALD PENNINGTON and KEITH THOMAS, editors. *Puritans and Revolutionaries: Essays on Seventeenth-Century History Presented to Christopher Hill.* Oxford: Clarendon Press. 1978. Pp. xii, 419. $32.50.

To all students of seventeenth-century English history, Christopher Hill needs no introduction. Always inspiring, though frequently controversial, Hill has dedicated himself to the teaching of and writing on the "century of revolution" for more than forty years. To mark the occasion of his retirement from the mastership of Balliol College, Oxford, this festschrift has been prepared in his honor by a group of fifteen scholars, all of whom were, at one time or another, Hill's students and some of whom have become leading authorities in the field. Each of the fifteen articles included in this volume deals with a subject or theme that has interested Hill, and this is a work of historical scholarship *par excellence*.

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Although every one of the articles defies strict classification, they may be placed, for the sake of convenience in this review, into three groups: seven belonging to the history of ideas, three to social history, and another five to the political history of the revolution.

In this first category, A. E. Aylmer's "Unbelief in Seventeenth-century England" is an elucidating study on an intractable subject in intellectual history. With well-defined analytical questions in mind, Aylmer reveals both the nature and the changing patterns of religious arguments against atheism from the sixteenth to the seventeenth century. Nicholas Tyacke's "Science and Religion at Oxford before the Civil War" is a solid case study in an important historical subject. After a thorough analysis of the scientific development at Oxford in the decades before the Civil War, including the years of Arminian ascendancy under Archbishop Laud's chancellorship, Tyacke convincingly concludes that the correlation between Puritanism and the rise of modern science is a negative one. Similarly, the relationship between Puritanism and legal thought is the subject in Wilfred R. Prest's "Art of Law and the Law of God: Sir Henry Finch (1558–1625)." Although in his early life the young Presbyterian barrister attempted to unite the two realms in his millenarian writings, Finch's later career clearly shows that the legal mind had by the seventeenth century achieved an autonomy free from religious influences. Prest's article sheds much light on the failure of the Puritan schemes for law reform and the increasing antagonism between the lawyer and the Puritan during the revolution. In his "Puritanism and Democracy, 1640–1642," Brian Manning examines the democratic implications in Puritan thought as well as the limitations of democracy among the various Puritan groups. Perhaps this subject needs to be studied further in the light of grass-root democratic tendencies. Popular elections of ministers were quite common in London, and, even in a Presbyterian parish such as St. Mary Aldermanbury, where Edmund Calamy was minister, both the select vestry and the electoral requirement of literacy for parish offices were abolished. Keith Thomas successfully corrects a long-held misconception of Puritanism in his "Puritans and Adultery: The Act of 1650 Reconsidered." Far from being a product of "an excessive outburst of Puritan zeal," the act was the result of a long-term movement in English life during the preceding century. Other articles in this categroy include K. H. D. Haley's study of Dutch millenarianism and Robin Briggs's study of the Jansenists in France.

In the area of social history, Peter Clark's "Alehouse and the Alternative Society" tells a fascinating story in regard to the socioeconomic causes of the tremendous increase in number of alehouses in Tudor-Stuart England and their threat to the traditional social order. Yet, in spite of the common fear, the alehouse never really became the medium for mobilizing popular radicalism. In her "Puritans and Poor Relief: The London Workhouse 1649–1660," Valerie Pearl newly appraises the Puritan attitude toward the poor, a subject that has long been misunderstood. It is an absorbing study of London hospitals, the plans of Samuel Hartlib, and the founding of the Corporation of the Poor in London. The vocational and educational features of the latter are beautifully illustrated. In the light of what can be known from London parish records, Pearl's picture is closer to truth. Richard Grassby's "Social Mobility and Business Enterprise in Seventeenth-Century England" is indeed a masterpiece in social history, which gives a penetrating analysis of the dynamic social equilibrium between the landed interest and the business community in early modern English society.

Of the remaining five articles, C. M. Williams's study of Henry Marten does much to reveal both the intriguing personality of the man and his strenuous career. David Pennington has a fresh look at the making of the Civil War in 1640–42, not from "what the conflicts were about" but "why at one stage an armed conflict began." In his "'Honest' Radicals in the Counties, 1642–1649," David Underdown offers a new helpful framework for the study of the English revolution, which, according to Underdown, was not one but a threefold revolution: the revolution of the moderates in 1640–42, the more violent revolution of 1648–49, and the abortive revolution of the Levellers and sectarians. Underdown's essay deals with the second. In using Thomas Burton's *Diary*, Ivan Roots gives a detailed analysis of the tactics of the Commonwealthsmen, especially Sir Arthur Hesilrige, in frustrating the plans of the government in the early weeks of Richard Cromwell's Parliament. The general situation of Wales in the twenty years of the revolution is surveyed by A. M. Johnson.

For many of us who know Hill mainly through his scholarly works, the personal reminiscences contributed by his former colleagues are revealing. The bibliography compiled by M. F. Roberts of Hill's writings from 1938 to 1977 testifies to his productive career in the past forty years. And all students of seventeenth-century English history will, of course, expect more to come from the masterful pen of the retired master in the many decades in the future.

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ROBERT ASHTON. *The English Civil War: Conservatism and Revolution, 1603–1649.* (Revolutions in the Mod-

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ern World.) London: Weidenfeld and Nicolson. 1978. Pp. x, 453. £10.00.

J. R. JONES. *Country and Court: England, 1658–1714.* (The New History of England.) Cambridge: Harvard University Press. 1978. Pp. 377. $15.00.

The simultaneous appearance of two surveys of the politics of seventeenth-century England, each devoted to one half of the century, naturally raises hopes that we now have a new synthesis to introduce students to this crucially important period. There is, to be sure, a regrettable hiatus between the two books, but it is a brief one, covering only the years 1649–58. Expectations of a unified treatment of the century are encouraged by the two authors' association as colleagues at the University of East Anglia. Both are skilled and experienced historians who have made substantial contributions to their fields in earlier works.

Alas, we find no unified treatment, and one book succeeds while the other fails. Both Robert Ashton and J. R. Jones are writing political history. But Ashton relates the politics to the social and cultural context (he makes an interesting excursion into the conflict of two cultures in the years before 1640, for example) and perceptively analyzes the connections between national events and the often very different concerns of the localities. Jones, however, eschews analysis and confines himself entirely to a surface narrative of high politics, to Westminster and Whitehall, war and diplomacy. He has a sketchy, inadequate chapter on "Social Trends," but it bears virtually no relation to the rest of the book. The impressive cultural and intellectual achievements of his period are ignored altogether: nothing on Wren, Purcell, or Newton, a single sentence on the Royal Society.

There is another striking contrast between the two authors. Ashton assumes a reader capable of dealing with conflicting interpretations and appreciating that history is less a recital of facts than a series of debates on the meaning of those facts. Jones, on the other hand, presents a history that is an apparently agreed narrative, evidently assuming a reader below the threshold of historical literacy needed to cope with disagreement or challenge to received opinion. Those who persevere as far as the concluding bibliographical essay will find some hints of the alternative approaches ignored in Jones's text, but little enough even there, and there are no footnotes. These deficiencies are evidently not the result of editorial policy, for W. A. Speck's *Stability and Strife* (1977), which covers the period immediately following Jones's in the same series, does not exhibit them.

Both authors are writing, presumably, for what in America amounts to an advanced undergraduate audience. Ashton's book is at the right level for this kind of reader. It assumes some previous exposure to English history, but once the necessary guidance is provided the student will always be challenged and stimulated. Jones's tedious political chronology, however, will stimulate and challenge nobody. The conceptual framework of court and country implied by his title does not infuse the narrative and is never systematically explained. There is a pressing need for a competent modern survey of the post-1660 period, but an unrelenting torrent of names and dates is not the way to satisfy it.

Ashton's book repays more serious attention by both the beginner and the student already familiar with the period. The interpretation of the early seventeenth-century conflict presented here differs radically from that in the most recent synthesis to cover the same ground, Lawrence Stone's *Causes of the English Revolution* (1972). Ashton's analysis of social change is less provocative than Stone's, but his alternative paradigm (which he shares with some of Stone's other recent critics) deserves to be taken seriously. The stress, as usual, is on monarchy, privilege, patriarchy, and deference: all concepts hard to eliminate from this period. The "opposition" spokesmen of the 1620s and the parliamentarian leaders of the 1640s are depicted as instinctively conservative, and the Puritans once again turn out to be defenders of tradition against Arminian innovation. A respectable case can be, and has been, made for this view, and Ashton always plays fair by acknowledging that there are other possible interpretations. He is particularly effective in showing how national issues were perceived through the lens of localism—another familiar theme but treated with freshness and originality. And as we should expect from him, Ashton offers a complex and convincing analysis of the ambiguous roles of London's various "concessionary interests." The narrative chapters on the 1640s are heavily dominated by parliamentary politics, of which they provide the best summary that exists in a book of this nature.

Ashton's reader will be exposed to a solid discussion of early Stuart government based on the patriarchy-deference model of society, a careful examination of the tension between centralization and localism, and a fine account of Long Parliament politics that draws on much recent research but is also well grounded in primary sources. That reader, however, may still be somewhat puzzled that a revolution, albeit an almost accidental one, occurred in 1648–49. For Ashton's assumptions about the nature of English society preclude more than a cursory exploration of the popular dimension f the revolution. People below the rank of the gentry are summarily dismissed from the "political nation" (p. 84); allegiance in the civil war is normally determined by the "instinctive loyalties" of man to lord, being for most people "something for their betters to de-

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cide" (pp. 173–74). Yet the facts he cites about the inability of peers like Derby to raise their tenants might suggest that the matter is worth pursuing further. Ashton regards Parliament's alleged encouragement of popular violence and religious radicalism as being important mainly because it enabled the Royalists to pose as the party of order and thus win over many of the wavering gentry. He stresses Parliament's vulnerability to such propaganda, without considering the possibility that popular support may have had something to do with Parliament's winning the war. When he confronts a grassroots movement on its own terms, Ashton wisely suggests that we can learn as much about popular mentalities from the conservative Clubmen as from the more familiar Levellers. True enough, yet one might still welcome a more systematic treatment of the radical minority.

Ashton's political nation may be defined too narrowly, but his is still a thoughtful, readable, and important book, which provides an excellent introduction to the political history of the period. *The English Civil War* is everything that *Country and Court*, unfortunately, is not.

DAVID UNDERDOWN
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JULIAN H. FRANKLIN. *John Locke and the Theory of Sovereignty: Mixed Monarchy and the Right of Resistance in the Political Thought of the English Revolution.* (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1978. Pp. xi, 148. $15.95.

Two themes in this stimulating book are of special interest. One of them is the argument that the theory of sovereignty found in the writings of sixteenth-century French constitutionalists differed significantly from that theory as discussed by their counterparts in Stuart England. The distinction is best appreciated by consulting the author's earlier *Constitutionalism and Resistance in the Sixteenth Century* (1969), which centers on Huguenot theories of resistance. Such theorists as Hotman, Beza, and Mornay advanced great claims for the Estates-General, urging that its powers properly included both the control of lawmaking and the administration of the government. The French king was at best a responsible executive, subject to deposition for cause. In Julian H. Franklin's view, the claim to depose the king is central to a genuine theory of resistance: without it no theory is worthy of the name.

But if resistance theory in sixteenth-century France favored the legislative body, its course in Stuart England was to take a different turn. After the middle of the century English theories of resistance contained a new radical element when it was urged by a handful of theorists that the ultimate control of government rested not in the two houses of Parliament but the general community. The latter might replace the king and even alter the form of government. The new version of sovereignty is traceable to an obscure Puritan divine, George Lawson, whose ideas were in his *Politica sacra et civilis* (1660). There they might well have died had it not been for John Locke, who incorporated them into his masterly *Second Treatise*. Franklin is explicit about the intellectual relationship when he writes: "Lawson's work was to be of great importance for the development of political ideas because it was taken up by Locke, and through him *alone* [italics added] transmitted to the future." And he adds that this decision by Locke makes him stand out among his fellow whigs as a genuine radical—an estimate with which this reviewer agrees.

Of equal interest is another theme, this one explaining how the English civil war transformed resistance theory. For their own reasons the parliamentarians made no claim to depose Charles I; and even though they justified their resistance by invoking parliamentary supremacy, they affirmed his title and authority. To Franklin, this behavior constituted a basic contradiction in their position; and he finds an analogous contradiction in 1689, although he thinks that by this time the claim to parliamentary supremacy had become muted. At one point he seems to imply that it may even have been abandoned (p. 90). The Convention Parliament, rather than recognizing that James II was being deposed, talked instead of filling a vacant throne. In Franklin's view this meant that theorists wrote within the context of a mixed constitution, and he argues that within such a constitution there was only one reasonable solution to the problem of resistance. This solution was to vest sovereignty ultimately in the general community as distinct from Parliament (the two houses). The point is made repeatedly and is the source of his admiration for Lawson and Locke. They alone, it is urged here, developed a logical and consistent theory of resistance within the mixed constitution.

In other respects Franklin's touch is less sure. He perceives the whigs in 1689 as unwilling to admit the supremacy of the two houses within the constitution; and on first sight this perception seems reasonable. Yet the conclusion is too sweeping. By this time lawmaking was widely seen as the sovereign power in the constitution and the king commonly described as only a single member of a powerful trinity of lawmakers (king, lords, and commons). The evidence is plentiful that leading members of the Convention Parliament subscribed to this view. There were whig theorists who wrote openly of the two houses as possesssing two-thirds of the law-making power; and even if the Convention Parliament

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appears not to have used such language, its members were confident enough of their position to approve publicly a declaration of rights providing for a change in the succession while striking at the king's control of the important dispensing power. His legislative veto would soon be obsolete. Under these circumstances the language cited by Franklin (p. 115) is less indicative than one might think. In sum, the whole subject of what was thought about parliamentary supremacy in Stuart England has to be approached with more caution than is at times displayed here. Perhaps the real test was not the claim to depose the king but the willingness to take from him control of the law-making power.

CORINNE COMSTOCK WESTON
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CHRISTOPHER CLAY. *Public Finance and Private Wealth: The Career of Sir Stephen Fox, 1627–1716.* Oxford: Clarendon Press. 1978. Pp. xvi, 362. $32.50.

Christopher Clay has written a sound, thoroughly researched study that presents in ample detail what can be known about Sir Stephen Fox, but a mystery remains. Stephen Fox came from humble origins to amass one of the great fortunes of England. He held high court offices from the Restoration to the accession of Queen Anne, at a time when court office could mean political power; he sat for years at the Treasury Board during the crucial period when the Treasury began to establish itself at the center of public administration; he was both a public spending officer (paymaster of the forces) and a private financier during the financial revolution; he was a member of the House of Commons in those critical years when the house seized for itself a powerful new role in English government. He was widely respected for his dignity, honesty, public spirit, courtesy, and charm. Yet Clay cannot identify any point where Fox contributed importantly to the great developments in which he was involved. Mainly he sat calmly at the center of the vortex and accumulated money. Lots of it!

Clay's book sheds light on many subjects related to Fox, such as the administration of the royal household, development of the office of paymaster, the use of secret service money (mainly nonpolitical), and the founding of Chelsea Hospital. These tidbits are useful, but they are not significant in improving our understanding of the major developments of the age. Clay makes an exceptional contribution, however, in unfolding the process by which Fox accumulated his great fortune. Fox profited from the contradictions existing in a period which saw the transition from a system of finance that was essentially personal in the king to a system that was public and institutionalized. As a royal official Fox enjoyed the fees and salaries of office; as a private person he profited from lending the profits of office back to the crown. His purchases of land, which are fully presented by Clay, were less profitable than his offices and money lending and were made primarily for political advantage, social prestige, and to found a family.

Although Fox's gains aroused envy, he remained a respected figure and lived to an advanced age. He won the esteem of such keen judges of character as Charles II, William III, Pepys, Evelyn, and Wren. Altogether Clay has given us a rich, fascinating, and yet disturbing story, for something seems to be missing. In an age of great issues and bitter partisan strife, how could a man be in the thick of it but not of it? Clay does not answer that question. While others hammered out answers to the problems of the period, Fox minded the shop and made money. Perhaps he had the right idea after all!

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MARK GIROUARD. *Life in the English Country House: A Social and Architectural History.* New Haven: Yale University Press. 1978. Pp. v, 344. $25.00.

The author of this scholarly work is Slade Professor of Art at Oxford. He began his career as an architectural historian, specializing in the history of English country houses and more interested in the houses themselves than in the people who lived in them. Signs of a new and broader interest—that of the social historian—appeared in his large book on Victorian country houses, published in 1971. There Mark Girouard began to observe the sorts of men who built country houses and the sorts of ideas they brought to bear on the shape and contents of these houses. In the book under review, the social historian comes fully into his own. He does not, however, as the title might suggest, deal with the country-house life of all landowners, big and small. Rather, he deals with landed magnates in their rural palaces—houses on the scale of Blenheim, Chatsworth, and Longleat—and he covers a long span of time, from the Middle Ages to 1940.

The major theme of the book is the contest in the owner's mind between a house that satisfied his desire for pomp and power and a house that satisfied his desire for privacy and comfort. As Girouard explains, privacy and comfort steadily gained on pomp and power. The eighteenth century was the turning point. To get privacy, houses were built away from villages, servants set apart from their masters, and the sexes carefully segregated. The last resulted in some parts of the house becoming male areas (e.g., the smoking room) and other parts fe-

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male areas (e.g., the drawing room). It also resulted in the separation of male and female servants, thus leading to the employment of a vast number of women and girls. To get comfort, much was done to improve country-house plumbing, heating, furniture, and general technology, especially in the last quarter of the eighteenth century and after. As Girouard puts it: "An English nobleman of the 1820's or '30's, purged and refreshed after a visit to his water-closet and bathroom, reclining in his dressing room on a richly upholstered sofa . . . and able, whenever he felt like it, to tug a bell handle and summon a servant . . . had reached a pinnacle of luxury which was the admiration of all his European contemporaries" (p. 266).

Sometimes Girouard rushes in where social historians have so far feared to tread, thus mixing shrewd with doubtful statement—as, for example, when he discusses the nature of that peculiar partnership that made up the Victorian ruling class (p. 270). One doubts that the middle classes as a whole entered the partnership as he suggests, or that the partnership came into being so late as the second half of the nineteenth century. But this and other such matters are not meant to be taken as serious flaws in a book that is a pioneering work, richly illustrated, and a delight to read.

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GORDON JACKSON. *The British Whaling Trade.* Hamden, Conn.: Archon Books. 1978. Pp. xv, 310. $17.50.

This account of the history of British whaling is a very good one, fills a notable gap, and is likely to be the authority for a long time. Unusual for modern monographs, it is written in a clear and lively style that makes reading a pleasure, and the author, Gordon Jackson, knows just how much detail to provide to make his points, without belaboring the reader with accumulated fact and repetitive information.

The place of the British in the history of whaling is an important one, beginning at the end of the sixteenth century, with their only predecessors the Basques, and ending in the last decade, with their only successors the whalemeat-eating Japanese and the subsidized Russians. But within that long span of time, there were many fluctuations, and the British found themselves sometimes the international leaders, sometimes subjected to humiliating reverses and grave financial losses from more efficient international rivals.

The Dutch proved to be more efficient in the first trade, around Spitzbergen, than either the English Muscovy Company or its subsidiary, the Greenland Company, or their interlopers; and even when the trade was thrown open in 1670, it did not quickly revive in the face of Dutch, French, and German competition, and Britain's rivals were earlier to explore the ice of Greenland rather than the bay fishing of Spitzbergen. A new Greenland Company was founded and failed; a great scheme in connection with the South Sea Company was a huge financial disaster. Already, however, Britain's North American colonists were developing a successful whaling industry and taking sperm whales with their fine candlemaking oil.

It was the considerable bounties paid on a tonnage basis on ships in the whale fishery after 1750 that triggered off the first regeneration of British whaling, though this subsidy was marginal to the now increasing demand for oils in wool textiles, lamps, and lubrication. The trade was also helped by the wartime difficulties of the American colonists and declined for a time when the colonists returned to activity after the Seven Years War, only to flourish mightily after 1783 when the Americans became foreigners in British markets; and, despite a decrease in bounty, increased British skill and the development of the Davis Strait fishery brought great expansion about 1800. But development in Arctic waters was now paralleled by the growth of a southern fishery as far south as Brazil and the Cape of Good Hope, where the Americans had pioneered, and even in Australian waters.

The fluctuations in this changing industry continued, with the Americans apparently far ahead in the middle of the nineteenth century. But then came a series of technical changes—the steam-driven catching vessel, the steam winch, the harpoon gun, the explosive harpoon, in all of which the Norwegians were a main innovating force—and North European coastal waters fished from shore bases became important. Britain was the main market and provided finance, and, interestingly, an important entrepreneurial influence was the Anglo-Norwegian firm of Salvesen, operating from Leith. As the trade moved into the twentieth century the use of whale oil in soap and margarine became of great importance, and the English tycoon, William Lever, began to use it as technological improvement in processing allowed. Finally, we move to the great Antarctic boom of the interwar years, the factory ship, the dangers of the extinction of most of the whale species, the weak beginnings of regulation and conservation, the last fling of the British industry in the postwar period.

Jackson keeps technological, market, and entrepreneurial issues nicely in balance, and there are good tables and statistical appendixes. An important branch of that underresearched subject, the British shipping industry, has been skillfully and interestingly illuminated.

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GEORGE BOYCE *et al.*, editors. *Newspaper History: From the Seventeenth Century to the Present Day.* (Communication and Society.) Beverly Hills, Calif.: Sage Publications. 1978. Pp. 423. Cloth $20.00, paper $10.95.

Few subjects touch upon as many aspects of modern British culture as journalism. Yet histories of the British press have been limited too frequently to issues involving freedom of expression or to the exploits of famous editors and special correspondents. The economic and cultural factors that have shaped the fourth estate have, surprisingly, been overlooked, as have those elements of social control that enable newspapers to moderate and socialize radical challenges to the established order.

It is within such a sparse historiographical framework that *Newspaper History* represents an important addition to the literature. It is a collection of eighteen essays that, in the words of its editors, attempts to re-examine "conventional assumptions about the development of the press." The essays range broadly over time. They include a detailed analysis of ownership patterns in London and the provinces in the seventeenth and eighteenth centuries, a discussion of voluntary press censorship during World War I, and a stimulating critique of the editorship of J. L. Garvin of the *Observer*, from 1914 to 1942. The Garvin essay, by John Stubbs, is particularly noteworthy because of its lucid handling of the interplay between a great creative personality and an influential journal.

Mostly, however, the contributors to this book turn their attentions to the Victorian period, when the press was transformed from a quality product of limited appeal to one that affected the lives of millions of people. Between 1830 and 1900 the average circulation of newspapers increased many times over, the retail price of a daily journal fell from 7d. to 1d. or 2d., and, perhaps most significant for the future, the number of journals increased spectacularly. It is this unprecedented expansion of the press—what Raymond Williams, one of the contributors, refers to as the creation of "a highly capitalised market product for a separate 'mass' readership"—and the parallel attempt to control the popular press by shaping "respectable" opinion with which most of the essays are concerned.

Among the best contributions in this area are those by George Boyce, James Curran, Alan Lee, and Virginia Berridge. Boyce scrutinizes closely the concept of the fourth estate and finds it to be mythical. The press, he argues persuasively, was not the independent mediating force between public opinion and the governing bodies of the country that it purported to be; rather, it was an institution that reflected political and economic pressures. Curran buttresses this theme by analyzing the effects of technological and commercial developments on the press, especially in advertising. He concludes that these changes helped to create a popular press that "emaciated and diluted the ideology of the earlier radical press," and by so doing contributed directly to "the integration of the working class into the political system." In an essay that is especially commendable for the quality of its scholarship, Lee documents the important link between proprietorial patterns and the expansion of readers. Finally, Berridge, breaking new ground, demonstrates the crucial role played by the Victorian Sunday papers in the transition from an essentially personal, political kind of journalism to one that was "popular" and nonideological in the worst sense. Each of these writers—and several other contributors—illuminates the role of the press in the shaping of mass culture, particularly as an agency of social control.

Much more remains to be done before a definitive history of the British press can even be attempted. We need to know, for example, more than we do about the evolution of journalism as a profession, about the precise readership and circulation of numerous journals, and about changing modes of journalistic expression, including sports and popular entertainment reporting, investigative muckraking, and, not to be slighted, the modern gossip column. *Newspaper History* provides a useful and challenging beginning to what will almost certainly be a fruitful source of activity for many years.

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PAMELA HORN. *Education in Rural England, 1800–1914.* New York: St. Martin's Press. 1978. Pp. xiv, 351. $25.00.

The evidence for the history of education in nineteenth-century England is abundant, and for twenty years now there has been ample literature on the subject. One expects new works to bring new evidence to light and to offer stimulating interpretations. Pamela Horn offers an addition to the field that could have filled a gap in the analysis of the Victorian educational scene, but the result is a disappointment.

The book does not define or analyze the rural school. The evidence used is traditional and anecdotal. Indeed, the study is poorly titled, for it is a review of the training and the work of teachers. This is an important subject and one that has received substantial attention before; the new material here, particularly under the title that is used, is not extensive, and no suggestion is made that the book overturns any previous interpretation.

The rural school deserves to be rescued from a romanticized obscurity and examined as a social institution in a contracting sector of industrial society.

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Horn has at least a dozen scattered references to the rural school, however it might be defined. In one section she tabulates "teachers in specimen country parishes" (p. 159) to show the degree of local recruitment, but there are no other attempts to collect evidence and offer generalized statements about the rural school. Only at the very end of the book are there references to some of the truly challenging questions related to the subject. The rural depopulation of the late nineteenth century, the concern for organizing rural teachers and for promoting "rural education" (pp. 256, 272) suggest themes that might have been pursued, but they are disposed of rather abruptly.

The general style of the book is a narrative, richly provided with citations from contemporaries, who are chiefly schoolmasters and school inspectors, with occasional reminiscences of students and reflections of novelists. In other words, the author's aim is to create a well-illustrated portrait of the schools and their students and teachers, set in a framework of the nineteenth-century legislation on education. The project is perhaps too ambitious, but its most serious defect is in losing sight of the title.

Horn is intent on describing the training, appointment, performance, and also the organization of elementary school teachers. This theme intrudes constantly, from a detailed account of the monitorial system, through training colleges, to tenure problems, "Bread and Butter Issues," and "Combination and Trade Unionism." These chapter headings indicate that the book is drawn some distance away from its original goal.

As an introduction to the subject of ninteenth-century education, the book would serve a useful purpose if it had stronger sections on the major investigative and legislative acts of the period. The highpoints of 1870 and 1902, however, are given brief treatment, even in the framework of the proclaimed scope of the book. The author's style and use of evidence is quite good, the book is well produced, and there are some valuable appendixes and a fair bibliography. The historian of education must still wait for a thorough treatment of the rural school and student.

RICHARD S. TOMPSON
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GILLIAN SUTHERLAND, general editor. *Education.* Introduction by P. FORD and G. FORD (Government and Society in Nineteenth-Century Britain. Commentaries on British Parliamentary Papers.) Dublin: Irish Academic Press. 1977. Pp. viii, 233. £10.00.

Education is a set of commentaries on the parliamentary papers. It includes essays on elementary education, fine arts and design, the British Museum, public libraries, secondary education, and scientific and technical education. The volume undoubtedly was conceived as a companion to the series of selected parliamentary papers published by the Irish University Press. This series has been an important aid to scholars, although, inevitably, the selection of papers has not satisfied everyone. Both Richard Johnson, in his commentary on elementary education, and Celina Fox, on the fine arts and design, mention Blue Books they consider important that have been omitted. It also seems odd and unfortunate that in a volume ostensibly encompassing education in Britain the entire topic of the universities should have been excluded.

The volume has a dual purpose. On the one hand, it is a straightforward bibliographical supplement to the Irish University Press series. For each topic, this volume contains useful, brief descriptions of the commission reports included in the series. This feature certainly will be of value to students. On the other hand, the volume has also provided an opportunity for each commentator to write an essay. Some, like Edward Miller (on the British Museum and public libraries) and Roy MacLeod (on scientific and technical education) have confined themselves narrowly to the origins, contents, and results of the parliamentary investigations reprinted in this series. Others, like Richard Johnson, have taken the opportunity to range widely over the entire subject, devoting as much consideration to the modes of education ignored by the commissioners and to their inherent biases as to the specific papers published by the Irish University Press. For practical uses as a bibliographical aid, it might have been better if the commentators had confined themselves to the more modest goals.

The most interesting essays are those that attempt to reconstruct in detail the circumstances leading to the calling of the various commissions and the results of their work. The Victorians themselves spoke of the commissions as if they were the spontaneous emanations of that mysterious "public opinion" they were so fond of invoking. Modern scholars would like to know the human agencies through which "public opinion" worked and the special interests that were represented. Gillian Sutherland's essay on secondary education performs this task especially well. Sutherland also demonstrates skillfully how the reports served to entrench a hierarchical system of secondary education with classical schools at the top.

All of the commentaries demonstrate our contemporary dissatisfaction with the Victorians. Johnson condemns their efforts for the education of the poor because they virtually ignored working peoples' opinions, while Sutherland condemns the tone of cozy self-congratulation apparent in their investi-

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gations of middle-class education. Neither the efforts that Fox shows were meant to improve the design and, therefore, the competitiveness of British manufactures nor the attempts to encourage scientific and technical education for industrial uses discussed by MacLeod saved England from economic decline in the twentieth century. Miller's commentaries on the British Museum and public libraries reveal unattractive xenophobia and unseemly personal rivalries among the reformers. Yet these justified criticisms ought not to blind us to the moral honesty to which these Blue Books testify. The Victorians were undoubtedly blinded by class-bound assumptions, and they were often maddeningly smug, but they left in their parliamentary papers an unprecedented record of their efforts and deliberations, uncensored and extraordinarily revealing.

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MEL SCULT. *Millennial Expectations and Jewish Liberties: A Study of the Efforts to Convert the Jews in Britain, up to the Mid Nineteenth Century.* (Studies in Judaism in Modern Times, number 2.) Leiden: E.J. Brill. 1978. Pp. xvi, 153. f 48.

Mel Scult's monograph is a rapid review of Anglo-Protestant attempts to convert the Jews of Britain. Before the French Revolution (e.g., during the readmission debate of the 1650s and during the debate over the "Jew Bill" of 1753) those who sought advantages for the Jews did so at least in part as a step toward their ultimate conversion. During the subsequent debates on emancipation, the opposite occurred—with one or two exceptions, those who advocated conversion were *opposed* to liberalization while those who favored liberalization were indifferent to conversion.

The most obvious flaw in this book is the abandonment of the theme suggested by Scult's title and preface. Scult adroitly traces the Christian belief that before the millennium Jews should have to be converted to the "true faith", but *only* in the seventeenth century did those who sought to aid Jews by readmitting them do so in hopes of attracting potential converts to hasten the Second Coming. A secondary weakness is that important elements of the story are either ignored or presented in a confused fashion. The chapter dealing with the background of readmission ignores most of the political and economic factors that influenced Jew-Gentile relations. More importantly, Scult neither mentions nor explains the fact that most British monarchs through ca. 1200 *opposed* Jewish conversion while actually granting Jews privileges denied to Christians. The discussion concerning readmission completely omits reference to the Marranos whose plight brought the issue to the fore. It also virtually ignores the role of Oliver Cromwell, who, despite his own commission's findings, chose to readmit the Jews, though apparently without any millennial expectations. The chapter on the "Jew Bill" of 1753 begins with the opposition to the measure, thus leaving the reader to guess at what arguments were being opposed. Sections of the chapter on the French Revolution, the millennium, and the Jews are very confused and nearly incomprehensible. Finally, the bibliography lists no item published since 1966, thus missing the important studies of Garrett and Hill, among others. Also absent is Hobsbawm's by now classic *Primitive Rebels* published in 1965.

Fortunately, the strengths of the book are also abundant. Scult has done his homework. Although the millennial attitudes of the seventeenth century come as no surprise, Scult does provide substantial examples of the pamphlet literature of the day. Similarly, the discussions on the "Jew Bill" and the emancipation debates are peppered with first-hand accounts culled from all of the available sources. Scult is also provocative and seemingly accurate when he challenges assertions of Thomas Perry, whose *Public Opinion . . . in 18th Century Britain* is the standard work on the 1753 incident. Where Perry maintains that the Tory opposition to the bill was not influenced by anti-Semitism, Scult demonstrates that it was; where Perry sees little if any conversionist tendencies on the part of Whig sympathizers, Scult effectively demonstrates that such was the case. Lastly, we are in Scult's debt for his suggestion to scholars that a rich vein of untapped evidence for the study of British and European Jewish life lies contained in the archives of the London Society for the Promotion of Christianity among the Jews.

In sum, this is a good effort that might have been better had Scult redefined his thesis, explored additional avenues of inquiry, and organized his thought more felicitously.

JOSHUA B. STEIN
Roger Williams College

RICHARD TOMPSON. *The Charity Commission and the Age of Reform.* (Studies in Social History.) Toronto: University of Toronto Press. 1979. Pp. viii, 279. $17.50.

The title of this book does not accurately inform the reader of its contents. Except for a discursive and pretentious introductory essay on historiography, the book has very little to do with the "Age of Reform." E. L. Woodward's volume in "The Oxford History of England" series dated "The Age of Reform" as the period from 1815 to 1870. Richard Tompson's essay is unlikely to convince many read-

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ers to extend the period to include the twenty-five years when England was at war with France, a period barren of major domestic reforms. The first three chapters of Tompson's book contains eighteenth-century materials pertaining to the organization of charity, specific governmental inquiries, and the nature of inquiries in general. The reader must wait for chapter four on page 94 before being informed about the "Enactment of the Charity Commission."

The Royal Charity Commission sat from 1818 to 1837 with only short periods of inactivity. During this long span of time it surveyed many of the 29,000 charitable trusts that had been created in previous centuries to promote the relief of poverty and to advance religion and education. Its semiannual reports filled thirty-eight volumes. Its minutes, correspondence, and miscellaneous records remain a massive volume of unassimilated manuscript material in the Public Record Office. The author is so overwhelmed by the mass of his literature that he fails to contribute to a better understanding of philanthropy in the nineteenth century. He has, on the other hand, chosen to describe the organization and procedure of the charity commission. Thus, he describes the apparatus of investigation rather than using the evidence as a means of understanding some aspect of English philanthropy that was so bountiful during the nineteenth century. One would like to say that this book is a useful supplement to David Owen's distinguished work, *English Philanthropy 1660–1960*, but the author, unfortunately, remains too undecided whether he is writing about charity, the charity commission, or the nature of governmental investigations.

R. G. COWHERD
Lehigh University

CARLOS FLICK. *The Birmingham Political Union and the Movements for Reform in Britain, 1830–1839*. Hamden, Conn.: Archon Books. 1978. Pp. 206. $15.00.

Thomas Attwood (1783–1856) is best known as a currency crank and organizer of the Birmingham Political Union, a short-lived movement that traditionally played a fairly important role in the passage of the Great Reform Bill of 1832 and later had some marginal influence upon the Chartists. In this study of the Birmingham Political Union, Carlos Flick traces its membership and history in a scholarly manner. In his conclusion he argues that the traditional interpretation is backward: the union was of little importance in the 1832 Bill but did affect the Chartists, though not in a way that much aided Chartism.

Attwood and his associates in reform tried to combine middle-class and working-class efforts behind peaceful political change in the direction of making the House of Commons more representative of the industrious people of the country. The middle-class members of the union, as described by Flick, were not a very inspiring group. Doctrinaire and moralistic, they were for the most part petty tradesmen who had a yen for change. The last sentence of the book expresses the truth about the Birmingham Political Union: "Attwood and his associates remained poor prophets to the end." Furthermore, as the author points out, the union tended to be isolated from other reform movements by the extreme provincialism that was then current in the industrial towns, many of which had grown from villages only recently. Birmingham was particularly isolated due to its concentration upon hardware rather than heavy industrial goods. Flick is sound on this point, but it is unfortunate that he does not seem to have used Donald Read's pioneering study, *The English Provinces* (1964). Nevertheless, the character and personality of the leaders of the Birmingham Political Union are brought out well here.

I am inclined to agree with Flick's conclusion, though he might have pointed out more clearly that Chartism was in debt for its principles to eighteenth-century middle-class reformers such as Major John Cartwright. As far as the Reform Bill of 1832 was concerned, it could succeed because it had sufficient respectable opinion behind it. Chartism, however, was doomed to failure from the start because of its predominantly working-class membership and because the country's leaders were not ready for democracy.

This book assumes that the reader has some knowledge of British political history of the period. The author makes few concessions to the nonspecialist. Students of reform in early nineteenth-century Britain, however, will have to reckon with Flick's narrative.

JOHN W. OSBORNE
Rutgers University, New Brunswick

DAVID F. KREIN. *The Last Palmerston Government: Foreign Policy, Domestic Politics, and the Genesis of "Splendid Isolation."* (A Replica Edition.) Ames: Iowa State University Press. 1978. Pp. viii, 252. $8.50.

The central thesis of this work is that the foreign policy of Palmerston's last government was determined by his perception of what would keep him in office. Palmerston understood the weakness of his support among the hodgepodge of Whigs, radicals, and Peelites he had gathered under the Liberal banner. He was acutely aware of public opinion and anxious to preserve the John Bull image he had

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cultivated for thirty years. To do that and to attract Conservative support, he had to pursue an active, assertive foreign policy.

That this wily old politician saw domestic politics and foreign policy as inextricably intertwined is no surprise. Nor is there anything new in David F. Krein's depiction of the other principals: the inept foreign secretary, Lord John Russell, with his absurd Whig dogmatism; the restless Gladstone, making his bid for radical support; the queen, conspiring with Granville and others to restrain "those two dreadful old men."

Despite the queen's opposition, a divided cabinet, and an inadequate army, Palmerston and Russell achieved a few popular triumphs in the early 1860s. Their successful Italian policy in 1860, their diplomatic victory in the Trent Affair a year later, and their ability to determine the succession to the Greek throne all strengthened the government at home. But these successes resulted more from the weakness of Britain's opponents than from British strength. Only in the Trent Affair was the government able or willing to support its policy with action.

The weakness of British policy was laid bare by the Polish insurrection of 1863. Unable to intervene and unwilling to allow France to do so alone, Palmerston resorted to bluff—the threat of joint Anglo-French intervention. The Russians, worried but unwilling to concede a vital national interest, crushed Poland and revealed the futility of Palmerston's bluster. In the following year, the Schleswig-Holstein humiliation signaled the bankruptcy of his policy. The result was that "Palmerston presided over the triumph of British insularity in a form as irresponsible and exaggerated as the kind of internationalism he had espoused" (p. 179).

This work clearly demonstrates the interaction of domestic and foreign politics, but it underestimates the ideological component of British policy. Palmerston undoubtedly wanted to stay in office, but he also wanted to prevent the expansion of French or Russian influence in Europe. His long-standing desire to clip the wings of the American eagle led him to encourage the French in their Mexican adventure. But his well-known hatred of slavery restrained the pro-Southern tendency of his own government. The ill-fated Polish bluff may have resulted, as Krein suggests, from Palmerston's regret at his inability to aid Poland earlier in his career. Although domestic political consequences were never absent from Palmerston's mind, to attribute his policies solely to them is somewhat simplistic. It is also strange that a work that emphasizes the domestic roots of British policy should largely ignore the press and public opinion, though Palmerston's concern for both is often mentioned.

This book is a shortened, "Replica Edition" of a doctoral dissertation. Based largely on archival sources, cogent and tightly written, it is an admirable example of the advantages of this form of publication. It provides a convenient summary and a useful analysis of the last phase of Palmerstonian diplomacy.

DARWIN F. BOSTICK
Old Dominion University

JOHN P. ROSSI. *The Transformation of the British Liberal Party: A Study of the Tactics of the Liberal Opposition, 1874–1880.* (Transactions of the American Philosophical Society, number 68, part 8.) Philadelphia: The Society. 1978. Pp. 133. $8.00.

Little enough has been written on the Liberal Party in the 1870s. With the exception of Irish questions, no monograph has yet appeared on Gladstone's first government. For an account of the subsequent period of Liberal opposition, from the electoral debacle of 1874 to the triumph and reinstallation of Gladstone in 1880, one can now turn to John P. Rossi's brief study. The colorful narrative illustrates the chronic dissension in the party—the overriding theme of the book—by examining Liberal performances in Parliament, especially the divided counsels as to the proper course of opposition to Disraeli's foreign policy. On the Eastern Question, for example, Rossi provides an admirable account of the Liberal parliamentary resolutions of 1877–78, which exacerbated Liberal discord and made no headway against the impregnable Conservative majority. There is also some electoral study, most interestingly in the case of three by-elections in 1879–80. Manuscript and periodical sources are interwoven with reliance on established authorities—R. T. Shannon on Gladstone's relationship with popular Liberalism, and H. J. Hanham and Trevor Lloyd on the two general elections. The overall result is a portrait of a sorely divided party. Between 1874 and 1878, Rossi writes, "the Liberal party was little more than a title searching for meaning" (p. 12).

Rossi is most reluctant to draw more than descriptive conclusions, but in many cases the material speaks for itself. It is clear that the party's Whig leadership, besides being disrupted by the "retired" Gladstone's frenetic campaigning, was temperamentally incapable of unifying the party. Hartington, the nominal Liberal leader in the lower house from 1875, curtly rejected overtures to place his leadership on a popular footing, consistently took positions in the rear of his potential troops, sulked at the general disregard of his authority, and finally abdicated to Gladstone in 1880. Nor was Hartington's supersession a mere matter of inevitable bowing to superior popular forces. The parliamentary revival of the party, which occurred during

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the Afghan wars of 1878–79, was spearheaded by Lawrence, Northbrook, Halifax, and Ripon—all Whigs with Indian experience. On the Afghan issue, as on all others, Hartington lagged well behind. He became Indian secretary in 1880. Such was the quality of Gladstone's appointments.

Gratuitous references to Gladstone's "honesty" might raise a few hackles, as does the casual standard of proofreading. What prevents this book from coming into sharp focus, however, is the disparity of its two concerns. On the one hand, Rossi studies the vitality of the Liberal Party, from the depths of 1874 to the "transformation" of 1879–80. On the other hand, he restricts his scope to the *foreign* issues that opposition leaders took up against the Conservatives in Parliament. Shortchanged are the domestic issues that animated advanced liberalism even in the heyday of jingoism—disestablishment, the county suffrage, and the agricultural laborers. What part did these issues play in the "transformation" of the Liberal Party?

MATTHEW R. TEMMEL
Seattle, Washington

PETER MARSH. *The Discipline of Popular Government: Lord Salisbury's Domestic Statecraft, 1881–1902.* Atlantic Highlands, N.J.: Humanities Press. 1978. Pp. viii, 373. $38.00.

Historians have largely concentrated on the third Marquess of Salisbury's impressive achievements in the realm of foreign policy, but in this thoroughly researched volume Peter Marsh argues that Salisbury's little-studied ability to marshal the forces of conservatism at home was a prime factor in the establishment and maintenance of the "conservative hegemony" of the late nineteenth century. It was "a story of discipline," Marsh suggests, in which the great prime minister tamed the passions of the mass electorate while adapting himself grudgingly to changing political conditions (p. 2). Committed to the rule of educated men of property, Salisbury employed a deft statecraft rooted in fear of social and political change to "stiffen the defences of the civilized order where alone he felt secure" (p. 10).

Concentrating almost exclusively on domestic political maneuvers at the highest level, Marsh authoritatively traces Salisbury's efforts to shape a powerful Conservative Party that would be able to resist an increasingly radicalized liberalism. With considerable skill, Salisbury consolidated his control over his party in the years after Disraeli's death in 1881. Marsh shows how Salisbury used an unwavering opposition to Home Rule to consolidate his power over the Conservative Party and to establish the alliance with Liberal Unionism that dominated British politics until his retirement. He emphasizes the political difficulties inherent in Salisbury's status as a peer and points out the importance to him of W. H. Smith as an ally and leader of the Commons. Although Salisbury resisted democracy as a threat toi good government, even civilization, he quickly learned the importance of molding public opinion through an efficient party apparatus, the "Middleton Machine." Several fine character sketches enliven the detailed discussion of party politics. Salisbury himself, interestingly, does not emerge as vividly as some of the lesser figures of the period.

More adaptable in his methods than in his objectives, Salisbury was determined from the first to adhere rigidly to Tory principles in the belief that in this way he could best attract those who feared liberalism. Interpreting the attempts by Peel and Disraeli to adapt in order to preserve as a willingness to "surrender . . . dearly held interests," he adopted an inflexible, defiant stance, which, by the end of the century, began to seem inadequate to younger Conservatives. Yet Marsh demonstrates that Salisbury's political heirs lacked his ability to balance the conflicting interests and concerns of the diverse membership of the Unionist alliance. Just two years after his retirement the great political instrument Salisbury had forged was in disarray over tariff reform, education, and other issues.

Marsh has written a solid and interesting political study. His emphasis on the most important politicians, however, limits his success in dealing with the more general problem of the maintenance of Conservative power. To have examined fully the political effects of such important socioeconomic phenomena as the agricultural depression and "villa toryism" would have been to write a different book—but there remain important questions: Did the skillful maneuvering of Salisbury "discipline" an otherwise unruly populace, or were forces beyond his control in fact shaping an increasingly conservative electorate? Were Salisbury's fears of the irresponsible masses unnecessary ones, and did England require not the superb delaying action of his "domestic statecraft" but imaginative leadership of a sort Salisbury could not, or at any rate would not, provide? Marsh's more traditional approach to political history does not, unfortunately, provide the answers to such questions about the context in which tactical decisions were made by the political grandees of late Victorian England.

GREGORY D. PHILLIPS
Lafayette College

ROYDEN HARRISON, editor. *Independent Collier: The Coal Miner as Archetypal Proletarian Reconsidered.* New York: St. Martin's Press. 1978. Pp. 276. $18.95.

The dust jacket claims that this book will usher in "a new genre in the study of labour history." It is

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supposed to have a "wide and radical purpose," to develop a "new mode of historical enquiry," and to challenge "conventional judgements." Indeed, many challenges are made: the reader learns that coal miners cannot be seen as "archetypical proletarians" working in large-scale enterprise and living in isolated, segregated communities. Many stereotypes of mining are discredited, including the old contrast between the National Association of Miners and the Amalgamated Association of Miners, which are shown to be more similar in their operations.

The book consists of eight essays, fitted together rather nicely by the editor. All of the contributors are associated with the Centre for the Study of Social History at the University of Warwick, where Royden Harrison is director. Three of the seven contributors are working on a doctorate and one is a candidate for an M.A. Several have actual experience in coal mining, and it shows.

The historical method employed is called "micro-comparative," and it lives up to its name. Maps, charts, tables, and meticulously detailed narrative focus on extremely small groups of miners over relatively short periods of time. The facts from these small, detailed mosaics rebel against the comfortable generalizations of labor history. This is almost atomistic history. One thing in this book's favor is that jargon is avoided and nearly all of the essays are written in a clear and flowing manner. Jargon added to the necessary technical vocabulary about mining would have been disastrous.

Article after article describes the Independent Collier (always capitalized in this book) hard pressed by increasingly large-scale enterprise, masses of workers new to mining, and ambivalent about union tactics. Two articles deal with the destruction of the colorful independence of the free miners of the Forest of Dean. Others deal with Scottish miners and particularly their difficulties in developing trade unionism. The furious struggles against Irish immigrants are presented in great detail.

The authors contrast mining communities in Lanarkshire and find them very different. They also seek to go beyond the union leaders and their supporters at the branch level to tap the experience of the rank and file. Comparisons are carried out to the extent of going from pit to pit. One figure who looms large in these essays is Alexander MacDonald, the moderate trade-union organizer who sought to embody miners' aspirations to be part of the labor aristocracy. His failure at working through arbitration and conciliation is described in detail. A counterpoint to MacDonald is offered in an essay on Edward Rymer, a little-known maverick militant.

The basic chronological span of these essays is across the nineteenth century, with much focus on the crucial 1870s. The essays sink a deep shaft and quarry a rather rich but narrow seam. The exacting technique is impressive.

HENRY WEISSER
Colorado State University

DUNCAN BYTHELL. *The Sweated Trades: Outwork in Nineteenth-Century Britain*. New York: St. Martin's Press. 1978. Pp. 287. $21.50.

This is a well-balanced, comprehensive study of the decline of the domestic system of production during the nineteenth century in Britain. Its author, Duncan Bythell, has previously written a history of *The Handloom Weavers* (1969). He displays a firm grasp of a wide variety of sources related to his subject. This is especially necessary for this type of work, which is largely a synthesis of old and new interpretations with little that is new in the way of original research. Bythell has made excellent use of relevant parliamentary papers for the period.

The first section of the book describes the key features of the main outwork industries and chronologically traces their rise and decline over the nineteenth century. The work is of particular value in reminding us and explaining why in certain sectors (e.g., tailoring and shoemaking) this type of industrial organization persisted well into the twentieth century. One shortcoming, which its author acknowledges, is the limitation of coverage mainly to England and the omission of detailed treatment of the Birmingham and Sheffield metal industries.

Bythell concludes that "protoindustrializers" like C. Tilly and R. Tilly, who hold that the putting-out system of the eighteenth century contributed to the acceleration of economic growth and the modernization of industry, have overstated their case. They tend to overlook the fact that not all nineteenth-century industries used outwork and that outwork persisted and in places expanded well into the nineteenth century. The author also rightly takes to task advocates of a return to intermediate technology, like E. F. Schumacher, whose *Small is Beautiful* (1973) suggested it as a remedy to unemployment and underemployment. Bythell feels that intermediate technology provided the nineteenth century not with a healthy, wholesome, idyllic past but with a life of insecurity and drudgery.

From the trade-unionist side, outwork or sweated labor was a thorny question. It was and is difficult to legislate out of existence. Workers themselves sometimes connived with the middleman to elude the inspectors. In Cradley Heath the low wages accepted by the outwork chainmakers threatened the men receiving relatively high wages in the Chainmakers' and Strikers' Association. It was only

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by the inclusion of women under the provisions of the Trade Boards Act of 1909 that Mary Macarthur and Charles Sitch were able to organize the women into the Hand-hammered Chain Branch of the National Union of Women Workers.

Throughout the work Bythell maintains a tone of fairness in assessing the conflicting schools of interpretation on various aspects of the question, and nowhere is this more evident than in his candid assessment of the courses, extent, and results of Luddism during the regency years. His comments on the bias of Arnold White and other witnesses on the Commons' Committee, which examined the state of the handloom weavers of 1834–35, is another case in point. It is hoped that the work will stimulate other students to discover further personal recollections, such as the diary of William Varley and the autobiography of Lucy Luck, to shed further light on the subject.

NORBERT C. SOLDON
West Chester State College

NORBERT C. SOLDON. *Women in British Trade Unions, 1874–1976.* Totowa, N. J.: Rowman and Littlefield. 1978. Pp. xiii, 226. $23.50.

The history of women in the British labor and trade union movement has been one of the most neglected aspects of historical research and writing. Norbert C. Soldon's book dealing with women in the British trade union movement between 1874 and 1976 is therefore a welcome addition to the bibliography on this subject. Like anyone else faced with writing on a long and complex subject, Soldon has had to make choices on what to include. Within the general economic and social history framework and through the growth of general and white-collar organization, the campaigns against "sweating," both world wars, the interwar depression, and the post-1945 boom, he has elected to concentrate on England, not Britain as a whole. He begins with the setting up of the Women's Trade Union League (WTUL), which marked the start of serious middle-class efforts to help women's organizations, and devotes the first two chapters or one-quarter of the book to it.

One of the book's great strengths is the presentation of not generally known details of the work and lives of individual women leaders, notably Emma Paterson, originator of the league, and, in the post–Second World War period, Margaret Mackay, the second secretary of the Trades Union Congress Women's Advisory Committee. Soldon also writes interestingly on women weavers, munitions workers in the First World War, and unemployment and low wages among women between the wars.

One of the major weaknesses of the book arises from this very concentration on detail. Because of it Soldon fails to bring alive the spirit of the times as it changes in response to alterations in working and living conditions in the century about which he writes. Another is the undue weight given to the middle class, which follows partly from his decision to start with the WTUL. Thus, in his brief introductory references to the period before 1874, he gives little sense of the initiatives of working women, other than the cotton powerloom weavers, in trying to organize; there is also little said of their tradition of combined action to maintain or improve working conditions and wages, which was the more remarkable because of the extent to which men succeeded in ousting them from jobs in the first half of the nineteenth century. This persistence produced a few major victories at the beginning of the period with which he deals, not just for women but for trade unionism generally.

Soldon's overemphasis on the middle and upper class is also apparent in his detailed treatment of the Open Door Council. The three pages (125–27) that he devotes to this topic might have been given to more important developments, such as the pressure put on the General Council of the TUC by active women members to help stop the decline in membership. There are also one or two minor mistakes, such as his unqualified statement on the final page that activist women trade unionists were "few in number" in the 1960s when, by the end of that decade, more of them than ever before were pushing for union jobs. The *Victoria Press*, although helped into being by Emilia Boucherett and Helen Blackburn through the Society for the Employment of Women, was started by W. H. Head (p. 7). And one very minor slip (p. 64)—the 1906 Sweated Trades Exhibition was opened by *Princess* Henry of Battenburg, strange as the title may seem, not the prince.

These criticisms do not diminish the large amount of very useful information that the book contains, including some excellent illustrations, and the light it throws on a number of aspects of the subject. Consequently, Soldon's work should find a place as a source book on the shelves of every individual and institution interested in the history of British women and the British trade union movement.

SHEILA LEWENHAK
Ruislip, England

GEOFFREY CROSSICK. *An Artisan Elite in Victorian Society: Kentish London, 1840–1880.* (Croom Helm Social History Series.) Totowa, N.J.: Rowman and Littlefield. 1978. Pp. 306. $22.50.

This well-constructed monograph provides a first-rate socioeconomic analysis of the upper strata of

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the working class in an area that was a center of industrial activity in engineering, metal working, armaments, and ship building. Beginning with the economy and the workplace, the author devotes a series of chapters to social structure, ideology, institutions, and politics. Appropriately, the book originated as a dissertation supervised by Eric Hobsbawm, whose important articles in the 1950s pointed the way to the new forms of social history that have been used so effectively by Geoffrey Crossick and others in the 1970s.

The labor aristocracy is the master concept around which the book is built. In his introductory and concluding chapters, Crossick deals suggestively with the views of some of his predecessors. Seeking a pluralistic position, he firmly rejects the "rigid economic determinism" of John Foster. He does not go quite as far as Royden Harrison or Robert Gray in the explanatory claims that he makes for the concept.

Despite his efforts to work out a more supple version of the labor aristocracy orthodoxy, however, Crossick has not been able to overcome the flaws inherent in the concept. Whereas he assumes that the generic characteristics of this "social formation" have already been established by previous research, the concept is at best problematical. In a classic article Hobsbawm argued that the labor aristocracy constituted a novel social formation, created by economic and social change in the middle of the nineteenth century, and that its emergence decisively affected the character of working-class politics and ideology in the second half of the nineteenth century. The thesis cannot be said to have been demonstrated; rather, it has been invested with plausibility by a series of studies, of which Crossick's is the latest. As A. E. Musson and others have pointed out, the discontinuity between early nineteenth-century artisans and mid-century "labour aristocrats"—so important to both the descriptive and explanatory components of the received interpretation—has never been established. Like his predecessors, Crossick slights the critical question of continuity and change.

Although Crossick rejects economic determinism and insists that "culture and values do not passively reflect economic structures," he has not been able to escape the limitations imposed by his assumption that "ideals and values are illusory things," to be explained by reference to a social reality that determines their essential content. Hence his interpretation of "labour aristocratic" values and ideology has a reductionist cast, despite the formal pluralism of his methodological pronouncements.

Originally formulated by Lenin as an explanation of the perversely social-democratic tendencies of the European labor movement during the First World War, the labor aristocracy concept has since inspired a good deal of solid research, including the book under review. From the very outset, however, the concept has been misleading.

TRYGVE R. THOLFSEN
Teachers College,
Columbia University

MICHAEL J. WINSTANLEY. *Life in Kent at the Turn of the Century.* Kent, England: Dawson Publishing. 1978. Pp. 236. $11.50.

It is not easy to bridge the gap between local and national history, especially when the local study, as in this case, is broad in scope, rather than pursuing a single theme. And to pursue historical questions from a wide range of sources, while at the same time presenting the fruits of a substantial oral history project in a style intended "mainly for the man in the street" and allowing "the people of Kent to talk in their own words about their everyday lives of two generations and more ago," can only compound the difficulties. It is a tribute to Michael J. Winstanley's ingeniousness that this book rings true and holds together.

Its primary appeal must be to readers with an interest in the history of Kent. Given its basis in a project that surveyed the recent past of the county as a whole, this seems right. And the way in which the substantial, and often vivid, extracts from interviews are situated within their social context, with discussions of Kentish agriculture, fishing, and the seaside towns, is, I think, the strongest feature of the book. This makes it all the more regrettable that the discussion could not go on to the inland market towns and suburban outposts of London. Without them, the book gives a somewhat lopsided picture of an early twentieth-century Kent in which two-thirds of the population lived in towns, and fishermen and farmworkers were of fast-shrinking significance.

Nevertheless, for the general social historian it is just where Winstanley most wholeheartedly pursues a special concern that he has most to offer. Certainly there are vignettes of all kinds here. Those interested in sexuality or the history of birth control in the lower working class, for example, will relish the admirably succinct account of Dover practices before 1914 given by an unskilled laborer's son: "The old man used to come home three parts cut on a Sunday from the pub and he'd say to the old woman, 'Let's have a lay down. All you kids out.' A few month's later, she'd say, 'You've done it again ain't you? You've filled the cupboard up again'" (p. 199). Elsewhere, women's historians can find a rare account of women brickmakers, historians of class relations some nice comments on clerical snobbery in Canterbury, and so on. On the whole, however, such nuggets have to be dug for: and it seems a pity

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that Winstanley's commentary does not pick up some of the general themes that recur in the interview material. One of the most promising would have been migration, both seasonal and long-term: the extraordinary range of work undertaken by traveling thrashers, brickmakers, or foreshore stake-net fishermen; the ebb and flow of the seaside trade, from the landlady down to the bathchair man pushing a gouty colonel down the front; or the first night out in town of the farm lad turned hotel kitchen boy. In short, this is less a book for history students who need guidance than for the self-directed scholar—unless the reader happens to be a Kentishman or woman.

PAUL THOMPSON
University of Essex

BRUCE HALEY *The Healthy Body and Victorian Culture.* Cambridge: Harvard University Press. 1978. Pp. 296. $15.00.

By focusing on the way Victorians thought about bodily (and its relation to psychological) health, Bruce Haley provides a fresh perspective on Victorian culture and a remarkable exercise in cultural history. The subject of "health" convincingly opens into a wide range of central Victorian problems: the well-known valetudinarianism of many famous Victorians, the equally emphatic preoccupation with sports and exercise of the Alpinists like Meredith, Tyndall, and Leslie Stephen, or the stupendous feats of pedestrianism by Dickens or Charles Kingsley; more importantly, Haley's study allows us to see such characteristic Victorian statements as Spencer's philosophy, Lewes's and Bain's psychophysiology, Newman's theory of education, Arnold's theory of culture, Maurice's theory of Broad-church Christianity, and Carlyle's theory of work as part of a coherent cultural awakening to the significance of the body in a world rapidly secularizing but unwilling to surrender the ideal of the spirit.

Without the theoretical and speculative density of Michel Foucault's studies of madness and disease, Haley convincingly demonstrates how the culture's attitudes toward health serve as metaphor and model for its fundamental values and dominant relations of power and class. The study is based solidly in documentary history, recording a coincidence of the Victorian obsession with health with the cholera epidemics that spread through Europe and England in the first half of the century. It traces the development of physiological psychology and physiology as a separate science and treats in some detail changes in the theory and practice of medicine. By doing so, Haley incidentally demonstrates that much that has looked to us like quackery was based on sophisticated modern views of the nature of the biological organism.

Moving through documents and texts, medical journals, newspapers, novels, and essays, Haley makes important connections. He shows that the ideal of Victorian manliness (and many shifts in fashion to expose and then disguise the human form) grew concurrently with the obsession with health. (We might retrace this obsession in our streets filled with joggers and health food stores filled with whole grains.) He shows how Thomas Arnold's new emphasis on the sound body to build the sound mind, emerging from the new awareness of body as well as from the Greek ideal, gradually transformed the Victorian idea of education; this emphasis was distorted frequently so that the body began to win out over the mind, and Matthew Arnold was driven to battle with the barbarians. Haley provides an important chapter on the history of sports, again anticipating much in our own culture and demonstrating the confusion in the culture's sense of the relation between mind and body. The Victorian distinction between "play" and "games" and between "amateur" and "professional" was deeply rooted in Arnold's kind of educational theory and in powerful class distinctions; it reminds us of how vestigial and class rooted our contemporary distinctions between amateur and professional remain.

If there is any weakness in the book, it is only a matter of proportion. The purely literary sources of this study emerge more densely at the end in excessively detailed readings of some Meredith novels. But much of Haley's close reading of central Victorian texts—if occasionally too elaborate for the study's purposes—cannot help shifting subtly our sense of them. Carlyle's *Past and Present*, Newman's *Idea of a University*, Arnold's *Culture and Anarchy*, for example, acquire a new historical density and urgency. The much-abused Charles Kingsley, if he remains as bluff and muscular as we remember, emerges as a more complex and interesting figure than his debacle with Newman can allow us to see; and we can see as well that even their famous feud was crucially connected to the problem of the relation between mind and body.

The point to emphasize is that this is a fresh and important contribution to Victorian studies and to cultural and intellectual history, opening new areas of exploration in the continuities between our culture and the Victorians' and offering us a new way to imagine the living sources of what might seem distantly intellectual problems.

GEORGE LEVINE
Livingston College, Rutgers University

G. R. SEARLE. *Eugenics and Politics in Britain, 1900–1914.* (Science in History, number 3.) Leyden: Noordhoff. 1976. Pp. v, 147. $17.00.

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Eugenics and Politics in Britain, 1900–1914 is a brief descriptive essay on the eugenics movement as an "instance of interaction between scientific investigation and political speculation" and as a symptom of the emergence of a new "Radical Right" in British politics (p. 2). Eugenics, G. R. Searle contends, became a sufficiently plausible basis for policy when, at the turn of the century, eugenicists drew support from August Weissman's theory of the continuity and unchangeability of the germ plasm, Francis Galton's biometrics, and Mendelian genetics. Searle sees the eugenics movement as part of a wider concern with national efficiency, a subject he has already discussed at length in his *Quest for National Efficiency, 1899–1914* (1971). Although somewhat ambivalent about the merit or persuasiveness of the eugenics program, Searle recognizes its politically conservative implications. Interest in the "scientific" encouragement of the best human stock, began with Galton's *English Men of Science: Their Nature and Nurture* (1874). Those writers following Galton's lead during the next four decades continued both to model their ideal type upon professional middle-class intellectuals and to lament the reluctance of professors and their kind to breed. Eugenics was, as Searle argues, a reaction against the emergence of social-welfare politics and the environmentalism of social reformers. Appealing to hereditarian arguments, eugenicists went beyond the reform tradition dependent upon individualism and self-help to a solution resting upon "scientific" or technocratic politics. And Searle rightly attributes the decline of prewar eugenics to increasing specialization in genetics.

While Searle traces the development and divisions within organized eugenic groups, such as the conflict between the Biometric Laboratory at the University of London and the Mendelian Eugenics Education Society, he does not address either the nature or contents of the controversies. Instead, Searle's description of what individual thinkers did and said about eugenics isolates these activities and concepts from the more general assumptions and purposes governing the lives, thought, and times of his spokesmen. Karl Pearson, for example, is discussed in only one dimension. Searle ignores Pearson's thoughtful philosophy of science, which was at odds with his devotion to genetic improvement. And we hear nothing about Pearson's contributions to statistics, which provided the method for collecting eugenic data, or about his post–Boer War conversion from committed socialism to a conservatism seeking social remedies through racial improvement. Searle's treatment of thinkers as if they were exclusively eugenicists is determined in part by his reliance upon evidence derived almost entirely from eugenics societies' sources. The eugenicists believed that their lack of effect was due to ill-considered attacks upon politicians or to vacillation about their possible role as politicians or educators. Searle accepts this judgment. But it can be argued, rather, that it was the eugenicists' simplistic, hereditarian assumptions that made their program unacceptable to the environmentalist social reformers who prevailed in politics and in the educational community.

The eugenicists and their opponents argued heatedly about hypotheses, data, methodology, and conclusions. An understanding of the part of eugenics in Edwardian intellectual and political life depends upon an analysis and explanation of the issues that provoked these controversies. Another book will have to deal with that.

REBA N. SOFFER
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A. JAMES HAMMERTON. *Emigrant Gentlewomen: Genteel Poverty and Female Emigration, 1830–1914.* Totowa, N.J.: Rowman and Littlefield, and Croom Helm, London. 1979. Pp. 220. $19.00.

Decaying gentility and unemployable gentlewomen constitute a popular literary stereotype in nineteenth-century England. The plight of "redundant" single women was regarded with increasing seriousness by Victorians, although much of the discussion was clouded by the issue of the traditional domestic position of women. In investigating the plight of these surplus women, A. James Hammerton has analyzed an important outlet for them in the various emigration schemes elaborated in the mid-nineteenth century.

The first organization to confront the problem of these underemployed women was the Female Middle Class Emigration Society, an avowedly feminist association, founded in 1862. The attempt to facilitate the emigration to the colonies of middle-class women was part of the feminists' campaign to expand educational and employment opportunities for women. But antifeminists also advocated emigration as an antidote to "redundancy," for they analyzed the problem of single women in terms of the simple demographic fact that the women outnumbered the men in Britain. The best solution, then, seemed to be to ship some women off to the colonies where there was an excess of unmarried men.

In spite of the attempt to dump these women in the colonies, female emigrants were far from helpless victims. Hammerton argues that conditioned dependence did not prevent these "distressed gentlewomen" from risking their reputations and defying social convention in searching out better lives for themselves. This phase of emigration for "distressed gentlewomen" was short-lived, and, by

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1870, larger numbers of women were emigrating from wider social backgrounds. While a good upbringing was the quality solicited by the emigration societies, education and domestic training were more to the point in making emigration work to their advantage.

Thus, while Hammerton's argument that the emigrants defeated the attempt of Victorian society to make casualties of them is compelling, his insistence that these "gentlewomen" were typical of some broader trends is less convincing. Drawing on sources that do not clearly identify social origins, Hammerton's assertion of these women's "gentility" is sometimes questionable. Because his examples of emigrant success stories are few enough to be exceptional, moreover, it is difficult to see that they can tell us "a great deal" about the fate of nineteenth-century English women (p. 193). These female emigrants, nevertheless, are interesting for what they are, an exceptional solution to women's limited occupational opportunities in nineteenth-century Britain. The female emigration societies were an important part of feminist, philanthrophic, and governmental reform programs, and the history of these societies forms a significant part of Hammerton's study. Finally, the notion of the "civilizing mission" of women articulated by enthusiastic imperialists—developed more fully in a recent article by Anna Davin on "Imperialism and Motherhood" (*History Workshop*, 5 [1978]: 9–65)—is a fascinating theme, despite, as Hammerton points out, "its increasing irrelevance to the actual course of female emigration" (p. 189).

THERESA M. MCBRIDE
Holy Cross College

OLIVER RANSFORD. *David Livingstone: The Dark Interior.* New York: St. Martin's Press. 1978. Pp. x, 332. $18.95.

Since his death in 1873, David Livingstone has enjoyed unabated appeal as a subject for biographers. Upwards of one hundred full-length posthumous treatments of his career have been written, and the recent centenary of his passing evoked a veritable deluge of books aimed at capturing the essence of a complex and frequently misunderstood personality. Yet for all this outpouring of biographical endeavor, Livingstone has remained an elusive subject. Early treatments such as W. G. Blaikie's official life were little more than extended exercises in hagiography, whereas more recent works, notably Tim Jeal's best-selling *Livingstone*, frequently have been characterized more by iconoclasm than insight. Interpretative biases aside, other major obstacles long have faced students of the renowned missionary-explorer. Chief among these have been the lack of an adequate guide to the welter of printed material by and about Livingstone, and more importantly, the state of splendid disarray that has typified extant manuscript sources bearing on his career. One of these problems has now been rectified, and a solution to the other is imminent. The present reviewer's *Dr. David Livingstone and Sir Henry M. Stanley: An Annotated Bibliography* (1976) and B. W. Lloyd and J. Lashbrook (compilers), *A Bibliography of Published Works by and about David Livingstone, 1843–1975* (1978) offer guidance through the mass of existing published information, and the ongoing work of the Livingstone Documentation Project is approaching fruition. This undertaking, an outgrowth of the centenary observances in 1973 that is centered at the National Library of Scotland, is designed to bring together in a single collection originals or copies of all extant Livingstone manuscripts. With the impending appearance of its catalog of documents, the groundwork will have been laid for a definitive biography of Livingstone. Meanwhile, Oliver Ransford, working without the full benefit of any of these tools, has written an intriguing biography that will perforce loom large in future Livingstone studies.

Quite simply put, his book, for all the controversy its central thesis is certain to arouse, is the best biographical treatment of Livingstone to date. "The Dark Interior" of the subtitle alludes to motivational forces within Livingstone rather than being a Eurocentric reference to the continent that was the focus of his life's work. The author indicates that his principal purpose for writing the book is to examine "the ambiguity displayed by this remarkable man" (p. 1), and he explains Livingstone's well-known behavioral fluctuations as those of a man who was prey to a manic depressive disorder that at times verged on the psychotic. He contends that this affliction was a hereditary disease known as cyclothymia and that a lifelong analysis of Livingstone's behavior makes manifest the manner in which he experienced the phasic "ups" and "downs" of the malady. Ransford, whose diverse qualifications include a missionary background, an M.D., long-time medical practice in Africa, and considerable accomplishments as a historian (he is the author of several earlier works on Africa and the present book originally was written as a Ph.D. thesis at the University of Rhodesia), builds a quite convincing case for his novel approach.

After an intelligent discussion, readily understandable to the nonspecialist, of the medical and psychological factors involved, Ransford pursues his thesis through a standard chronological examination of Livingstone's life. Numerous examples of both aberrant behavior and exuberant achievement are described as being products of the cyclical pattern characteristic of persons affected by the mal-

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ady. The author's close adherence to this theory results in some imbalance. For example, one wishes for closer scrutiny of Livingstone as a progenitor of imperialism and as a field scientist. Moreover, the book badly wants a concluding chapter or perhaps a timetable to tie the alternating periods of depression and hypomania together in an intelligible whole. Nonetheless, this is an exceptional piece of psychohistory with methodological implications that far transcend its subject field. Ransford's is not the final or definitive word on Livingstone, but subsequent studies will have to devote careful attention to his biographical interpretation.

JAMES A. CASADA
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F. W. E. SMITH, LORD BIRKENHEAD. *Rudyard Kipling.* New York: Random House. 1978. Pp. ix, 421. $15.00.

In 1945 the late Lord Birkenhead embarked on a biography of Kipling and was given full access to the family papers, under terms that gave control over publication to Kipling's surviving daughter, Mrs. Bambridge. When he submitted his draft to her in 1948 she imposed a complete ban on its publication, paying him the sum of £5,000 in compensation "on condition that he made an undertaking never to publish or cause to be published any biography of Kipling" (p. 4). In 1951 C. E. Carrington took over the role of official biographer; his book *Rudyard Kipling: His Life and Work* appeared in 1955; and the third revised edition of 1978 has an appendix on the circumstances in which he undertook the task. This should be read alongside the account of the dispute with Mrs. Bambridge offered by the present Lord Birkenhead in his introduction to the work under review. Mrs. Bambridge's death in 1976, following the author's in 1975, made possible the publication of "the long-suppressed biography," as it is described on the dust jacket, though the work now published is not that originally submitted for her approval. Lord Birkenhead, we are told, revised it extensively in the course of the 1960s, as indeed is evident from his quoting Carrington at several points and from his drawing—not always felicitously—on criticism published in that decade.

The fact of its suppression, which was widely known, led to speculation that the book contained damaging revelations, personal or political; but it is evident now that this was not the case. Indeed it comes as something of an anticlimax. After Carrington's more scholarly biography, Lord Birkenhead's seems lightweight. Its main interest lies not in the author's own narrative or commentary but in the passages he quotes from unpublished material, his selection from which often differs from Carrington's, and in the reminiscences that he collected from men and women who knew Kipling, some of whom had died before Carrington embarked on his research.

This incidental interest is considerable, but the work is marred by careless scholarship. There is, for example, no reference to Kipling's long friendship with Rider Haggard; there is an odd description of Andrew Lang as one of his enemies; *The Wound and the Bow* is cited as *The Arrow and the Bow*; a collection of essays is attributed *in toto* to one of the contributors; there are too many quotations with no sources given and too many unconscious unacknowledged parrotings of things said by other writers. More fundamentally, the book reveals a radical uncertainty in its estimate of Kipling as a man and as an author. Although professing sympathy, Lord Birkenhead often reveals by expressions of distaste or contempt a latent hostility to his subject. Where the writings are concerned, he has recourse repeatedly to citing views of other critics, while offering some paraphrases and incidental comments of his own. Neither the man nor the works are much illuminated by this process, so that T. S. Eliot's description of the original book as "insufficient" seems in retrospect well founded.

ANDREW RUTHERFORD
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GEORGE W. EGERTON. *Great Britain and the Creation of the League of Nations: Strategy, Politics, and International Organization, 1914–1919.* (Supplementary Volumes to *The Papers of Woodrow Wilson.*) Chapel Hill: University of North Carolina Press. 1978. Pp. xiii, 273. $16.00.

At the close of the First World War politicians and publicists eagerly claimed responsibility for the establishment of the League of Nations. This "great experiment," which would epitomize the positive response of Western liberalism to the world crisis of 1914–19, had a complex and confused paternity. Previous authors have produced pioneering studies that have illuminated the background of ideas and ideals from which it emerged, but, nevertheless, in Roland Stromberg's phrase, "uncertainties and obscurities" about the League of Nations have persisted. Everything is still not transparent, but George W. Egerton's book has cleared much of the ground. Building on the work of other scholars on both sides of the Atlantic, he has worked diligently through archives in Canada, Great Britain, and the United States to establish the process whereby diffuse and often conflicting ideas came to be transmuted into the League of Nations Covenant. He sees its final form as largely a triumph for the rather unlikely combination of Woodrow Wilson and Lord Robert Cecil. Many of the articles reflected notions

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that had become widespread in peace circles in the United Kingdom and the United States; the time was ripe to attempt a dramatic new international political and legal order. The high-minded center of British politics saw the possibility of making a reality of the war to end war. Other pacifists, however, were lukewarm in their response to an attempt to impose a system of collective security that would depend upon sanctions for its efficacy. The support given to the project by Lord Grey, the former foreign secretary, convinced many commentators that the "old diplomacy" was indeed dead. In office, Grey had skillfully used the league idea as a means of maintaining good relations with the United States. He appears again at the end of the story in his pathetic mission in Washington. Necessarily, however, Egerton's account concentrates on the balance of political forces and the decision-making processes within the Lloyd George coalition. His analysis is acute and generally sure-footed, although quite where the prime minister himself stood remains enigmatic. It is a situation in which the surviving archival material may lead to misleading assessments. Lloyd George was not a man for pen and paper, but Cecil showed no such inhibition. Even so, as the author fairly observes, there were cogent arguments advanced against the kind of league Cecil advocated. Lloyd George and advisers like Kerr and Hankey distrusted the sharp break with the past that so appealed to certain sections of the public. A limited league could be built up on the basis of conference diplomacy and functional cooperation. That was better than the structure of sanctions, guarantees, and obligations that could well place too great a strain on British resources. The Imperial War Cabinet was a good example of the kind of organization that had greater appeal. Lloyd George did not get his way—although, Wilson's objections apart, it is still rather puzzling why he did not marshal his case more effectively. Egerton hints that it might have been better both for Britain and the league if he had succeeded, but it is the merit of this well-argued and well-researched book that he leaves the readers to decide for themselves.

KEITH ROBBINS
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JOHN GRIGG. *Lloyd George: The People's Champion, 1902–1911.* Berkeley and Los Angeles: University of California Press. 1978. Pp. 391. $25.00.

There are at least two sorts of biographies: those that tell us primarily what the subject did and those that tell us what he was. John Grigg's *Lloyd George: The People's Champion* is of the latter variety. This is the second volume of a proposed four-part study of the long career of the most famous Welshman since Henry VII. The first, the widely acclaimed *Young Lloyd George*, carried the story through the Boer War years; the second continues through 1911, "the zenith of his pre-war career." The remaining two volumes will divide the rest of Lloyd George's life at the close of World War I. The plan is obviously to divide the long story at its high points rather than at more arbitrary times—elections, changes in office, or the like. It is a good scheme.

This book, then, is not about the election of 1906, the "radical" Budget of 1909, the Parliament or National Insurance Acts of 1911, or even about the foundation of the British welfare state. It remains true to its real subject: the much-misunderstood man behind these great episodes. Carrying forward the theme of his first volume, Grigg presents a very human Lloyd George. There are no cloven hooves, as there were for Lord Keynes; there are no halos and harps, as there were for Malcolm Thompson or the second Mrs. Lloyd George. Grigg's Lloyd George was a pragmatic politician, impressed by deeds and facts rather than by rhetoric and ideology.

The old Lloyd George myths must suffer in such a book, and they surely do. The disloyalty to Asquith and his other colleagues; the 1909 Budget as a red flag waved at lordly bulls; the "Lloyd George Memorandum" of 1910 as proof of disloyalty to party; and the abandonment of W. J. Braithwaite as proof of disloyalty to subordinates—there are so many hoary and ancient tales that have survived for so long. In each case, Grigg leads his reader through a well-documented and beautifully reasoned brief that redresses the historical balance considerably.

People's Champion is a fine book. It is the product of exhaustive research—many of the footnotes will become instant treasures to research students—and it is written with the elegance and wit for which its author is well known. It brings the reader closer to the enigma that was David Lloyd George than any biographical study done to this time. I do not doubt that it, and its three companion volumes when all are completed, will become the new "standard" life, against which others will be compared. No doubt, this will not satisfy the "Lloyd George industry," and the books will continue to appear quite regularly. I am sure Grigg and other stockholders in the "industry" would not have it any other way.

R. J. Q. ADAMS
Texas A & M University

PETER CLARKE. *Liberals and Social Democrats.* New York: Cambridge University Press. 1978. Pp. xiii, 344. $26.00.

DUNCAN WILSON. *Leonard Woolf: A Political Biography.* Assisted by J. EISENBERG. New York: St. Martin's Press. 1978. Pp. 282. $18.50.

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The wealth of recent writing on British politics in the late nineteenth and early twentieth centuries has generally focused either on tactical maneuverings by major politicians, or less often, on electoral analysis. In neither case has the role of ideas in politics been taken very seriously. Such Namierism has been found wanting even by eighteenth-century historians, only to be transferred to a later and less appropriate period. Peter Clarke's new book bucks current fashion to insist on the importance of ideas, particularly in the history of liberalism. Continuing the theme of his path-breaking *Lancashire and the New Liberalism* (1971), which argued persuasively for the viability of Edwardian progressivism, Clarke here demonstrates the intellectual viability of progressive thought (his argument dovetailing with that put forward simultaneously by Michael Freeden in *The New Liberalism* [1978]). His method is biographical (complementing Freeden's more strictly intellectual approach): Clarke examines progressive thought through the careers of four intellectuals—Graham Wallas, L. T. Hobhouse, J. A. Hobson, and J. L. Hammond—and shows how they, with others, developed in the prewar years a distinctive, innovative, and radical body of political ideas. "Liberalism as a permanent principle" was disentangled from particular expedients of property holding and given fresh life as a force for social reform. In seeking a society in which "the spontaneous fulfillment of capacities became possible for all," the progressives set themselves consistently and creatively (as in the use of fiscal policy) against conservatism. Yet their ideas, Clarke demonstrates, were neither simply a halfway house between traditional liberalism and socialism nor a watered-down version of that emerging socialism. They were acute in pointing out the limitations of the sectional, interest-based labor movement of the trades unions. Reform politics for them rested on hard and sustained thought, based in the last analysis on a moral appeal, and had always as its lodestar the *general* interest of the community. At the same time, against the bureaucratic socialism of the Webbs the progressives reaffirmed the fundamental values of liberty and democracy and the importance of "moral" over "mechanical" reform. "As the expert comes to the front and 'efficiency' becomes the watchword of administration," Hobhouse warned, "all that was human in Socialism vanishes out of it" (p. 71).

Writing with grace and assurance, Clarke brings his subjects back to vivid life and sets their development firmly in political and intellectual context. He provides the best account we have of how the "new liberalism" emerged out of the old, the way in which the "new liberals" renovated the liberal tradition, and the exact nature of their agreements and disagreements with others on the left. A half-forgotten generation has been restored to historical consciousness and respect by this sensitive and judicious work. Two weaknesses in Clarke's account, however, should be noted. First, we learn more than we need to of the details of his subjects' lives—the Hammonds's medical problems, for instance—and at times these minutiae get in the way of clear exposition and analysis of ideas. Second, and more important, Clarke (like Freeden) deals only elliptically with the question of influence on politics and policy. He seems to believe these progressives were influential in a major way and points to Winston Churchill's virtually total adoption of their ideas. The question, however, needs more thorough-going exploration, particularly since historians of early twentieth-century social reform like Bentley Gilbert have seen little of such influence. Of course, this may be faulting the author for not writing a different book. In any event, the importance of progressivism does not stand or fall with its success in influencing events: Clarke has helped expose the myth of an intellectually bankrupt liberalism doomed to decline.

The continuation of the progressive tradition in the post-Liberal era is evident in the career of Leonard Woolf. Duncan Wilson has written a brief account of Woolf's major political activities to provide background for the luminous autobiography that is his subject's enduring legacy. Wilson's book is indeed a useful supplement, particularly in his summaries of Woolf's political writings (comprising books, Labour Party reports, and a vast number of periodical articles) and his influential but little-known work as secretary of the Labour Party's Advisory Committees on International and on Imperial Questions. One consequence of this book is to reinforce Woolf's self-characterization as a "socialist of a rather peculiar sort" (*Beginning Again* [1964], p. 105). In his approach to politics and in his basic values, Woolf was a liberal socialist. For him, as for Clarke's progressives, politics was at root about ideas and ethics, and the necessary quest for social justice and welfare could never be allowed to trespass on the fundamental values of truth and liberty (which Woolf, like Wallas, Hobhouse, Hobson, and the Hammonds, traced to fifth-century B.C. Athens). He wrote in 1937: "There is no such thing and never will be such a thing as a dictatorship of the proletariat. A dictatorship is a dictatorship of dictators . . ." (Wilson, p. 196). These two books, Clarke's in particular, offer timely reconsiderations of an apparently failed tradition of political thought (most progressives, and Woolf, ended in varying states of disillusion) that may yet have something to say to a generation debating the viability of "Socialism with a human face."

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JOHN STEVENSON and CHRIS COOK. *The Slump: Society and Politics during the Depression.* London: Jonathan Cape. 1978. Pp. x, 348. £8.95.

This volume is an attempt at revising, or eliminating, what John Stevenson and Chris Cook believe to be established myths about the 1930s in Britain. Such myths include the exaggeration of the economic bleakness of the period; the failure to appreciate the buoyancy and resilience of the British economy before 1939; the 1931 general election disaster for the Labour Party as a direct consequence of MacDonald's betrayal; the overinflated influence of the impact of the hunger marches; the failure to recognize that political stability was the most important single characteristic of British politics in the years leading up to World War II.

It is, of course, correct to argue that popular belief—which may perhaps be described as mythology—still thinks of the thirties as "the Slump": the black years. But historians have long forsaken the more simplistic of popular illusions, and a quarter of a century ago C. L. Mowat published what is still the most detailed, as well as the most balanced, account of the period, in which the historical record was set down in ways that have not been altered fundamentally by subsequent research. The economic historians of the 1960s added to, without changing, our views on the sources of the economic recovery after 1932 and the marked rise in living standards for those who remained at work. Certainly, because of the wide gap between popular comprehension and the now received doctrines of the historians, there is a continuing need for new appraisals; but it is important not to claim too much. Whatever our final assessment of this volume, it is a curiously lopsided affair. There are chapters on the economy; on the social impact of unemployment; on the activities of the Communist left and on the fascist right; two psephological chapters; and a concluding chapter with the title "The Revolution that Never Was." Much has been left out, or only summarily discussed. There is almost nothing on mainstream politics, except the discussion of election statistics, which is quite useful; almost nothing on the trade-union movement; almost nothing again on social changes, including the position of women; and the documentation of social conditions is far from complete.

Conscious attempts to correct interpretations of the historical past do rather depend on meticulous and scholarly standards of approach and analysis; and these present authors do not always match the criteria one assumes they set themselves. Here are two examples out of many. In chapter five they attempt to assess the impact of unemployment and begin by considering contemporary attitudes. Arthur Bryant is quoted in 1937 as severely critical of Orwell's *The Road to Wigan Pier*, describing it as propaganda. "Thus," write our authors, "even for contemporaries, the picture was by no means clear." But to a majority of the middle and upper classes the picture was abundantly clear. Bryant was a conservative journalist and historian, and he was only the echo of accepted views about the unemployed—that many were shirkers, that work was available to those who really wanted it, and so on. One cannot really get very far with British history if the approach to class practice, and class ideology, is muted. The second example is of a specific historical event—the 1931 general election—for which the authors ride their revisionist horse with a very short rein. "The whole assumption," they write, "of a panic election has been rightly disputed"; and in evidence they proceed to quote a sentence from the *Manchester Guardian*, itself quoted by Bassett in his study of 1931 to the effect that "it has been a remarkably quiet election" with only isolated incidents of rowdyism (p. 102). There are several points here. The Labour Party in 1931 lost 215 seats and was pretty well wiped out as an effective opposition for the next four years. The parliamentary Labour Party of 1929–31 was almost annihilated. It might not have been a panic election, but clearly something dramatic had happened to a party that had been steadily increasing its electoral strength in the previous decade. But the *Manchester Guardian* quotation given above was the last sentence of a comment that began: "There is agreement that it was panic . . ."; and since the authors forgot to include the page reference to Bassett, it will be found on page 312, and in the issue of October 29, 1931 of the *Manchester Guardian*. This paper, by the way, is perhaps best known in this context for its comment on 1931 that it was "the most fraudulent election campaign of modern times" (quoted in Mowat, p. 411). It may be noted in passing that the election was relatively free from rowdyism; only a few former Labour ministers were howled down and occasionally were unable to speak at all. But this has been the trend in the twentieth century: the British, long famed for their violence at the hustings before 1914, have now shifted the location of their violence to, among other places, football stadiums—a matter of some import to the social historian.

This volume is not the text for popular consumption we have been waiting for; it is too much scissors and paste, too flip in its judgments, and too hurriedly assembled. We have still to return to Mowat.

JOHN SAVILLE
University of Hull

G. C. PEDEN. *British Rearmanent and the Treasury: 1932–1939.* Edinburgh: Scottish Academic Press. 1979. Pp. x, 227. £6.00.

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G. C. Peden has written an interesting account of the Treasury's role in British rearmament during the 1930s. Though the author reveals few surprises, one learns a great deal about the subtle ways in which the Treasury sought to exert its influence on rearmament policy, at least until late in the period, when the imminent threat of war led to a diminution in its power to control. On the whole the Treasury emerges from this study with more credit to its name than has been the case in previous studies.

There were two main problems with respect to defense in the 1930s: first, the need to increase Britain's military strength from the low level reached after a decade or more of retrenchment, and second, to accomplish this task without straining the country's financial and economic resources, which would have left Britain economically exhausted before war broke out. The declared policy was "to rearm while maintaining financial stability, so as to deter aggression while diplomacy removed the causes of war." In the event, the last objective was not achieved, but the steady process of rearmament, with concentration on air defense, did mean that Britain was in a reasonably strong position in 1939–40, which enabled her to maintain a holding operation until additional resources and aid outside the United Kingdom could be drawn upon, while at the same time the country's resources had not been stretched to their limit before the years of crisis. Considerable credit must go to the Treasury for ensuring that a sensible balance was struck between these objectives and also for its recognition that financing rearmament required resort to unorthodox financial methods, in particular raising loans. The Treasury's traditional financial caution did not impede orderly rearmament, at least not in the later years. In fact, if anything, real resources (for example, skilled manpower and factory capacity) threatened to be a more important braking force, and Peden expresses concern that more was not done to train the unemployed in skills that would have been suitable for the defense and export industries. It is difficult, however, to accept his implication that governments should have run more budget deficits to finance rearmament. To what purpose? Surely from his own account the balance between rearmament and financial capacity seems to have been just about right.

DEREK H. ALDCROFT
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NORMAN ROSE. *Vansittart: Study of a Diplomat.* New York: Holmes and Meier. 1978. Pp. xi, 308. $19.50.

Being dead right on the master foreign policy issue of their generation has not always spared professional diplomats the frustration of having their insights and recommendations set aside or maladroitly implemented by their political superiors. Such, it may be argued, was the experience of George F. Kennan, who saw his project for containing Soviet communism by the selective application of political and economic measures transmuted into a futile effort to control, often by main force, every ambiguous manifestation of destabilizing change in every quarter of the globe. And such, *mutatis mutandis*, was the experience of Sir Robert (later Baron) Vansittart, whose early and emphatic delineation of the "German danger" failed to persuade the cabinet of the imperative need to rearm the empire at top speed and to equip it with allies, including even the Soviet Union, which it would require to preserve the peace—or, more probably, to fight what Alan Bullock once called "The War That Had To Be."

Norman Rose's study of Vansittart's career improves on its principal predecessor (Ian Colvin, *Vansittart in Office* [1964]) in several important respects. It offers a fairly full and readable account of Vansittart's formative years, of his persistent literary interests, and of his rise to eminence at the Foreign Office. It treats the major episodes of his life in the 1930s with more insight, and appreciably less detail, than Colvin's quotation-studded work provided. It extends the coverage to include a useful discussion of the creation and performance of Vansittart's intelligence network and of his public activities during and after the Second World War. It combines admiration for his analytical powers and driving energy with enough critical detachment to forfend the impression of hagiography: Vansittart is a "flawed hero," something of a mandarin out of season, whose disdainful attitude toward the political practices of British democracy and underappreciation of the role of the press and public opinion in the making (and unmaking) of foreign policy led to costly errors of judgment. In short, it presents the best-proportioned and most thoughtful account of its subject now in print.

To explain Vansittart's failure to move the cabinet along the right lines of policy at the right tempo, Rose relies on a medley of personal and situational factors. He states, unexceptionably, that Vansittart's personal style—the qualities of mind and taste that set him apart as a "brilliant solitary"—made him a "difficult" subordinate and undercut his effectiveness with his less talented associates. As for the international situation, it was undeniably complex, unstable, and increasingly threatening to the overextended British Empire, and Vansittart was undoubtedly correct in seeing the management of relations with Germany as the key to Great Britain's survival as a world power. Profound as his insights into the dynamics of German nationalism might have been, however, they

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were vitiated by his inability to share the government's sensitivity to some of the "moralistic" aspects of international politics and its desire to integrate British interests into an emerging configuration of power that took greater account of the United States and of anti-imperial sentiment than Vansittart's habits of thought, decisively shaped in the heyday of British imperialism, could accommodate. Rose lightly limns this suggestive theme, which deserves further development.

DONALD LAMMERS
Michigan State University

JOHN E. TURNER *Labour's Doorstep Politics in London.* Minneapolis: University of Minnesota Press. 1978. Pp. xiv, 396. $19.50.

Recent findings by political scientists suggest that the increasing "nationalization" of British politics as a consequence of the advent of television has reduced both the significance and the vitality of constituency parties. The early years of the 1960s, when John E. Turner carried on the research on which this work is based, were perhaps the last during which Labour's local parties might be observed functioning much as they had in the years since their establishment in 1918. The study is based on three London Labour parties: Bermondsey, a safe Labour area, Fulham, a marginal constituency, and South Kensington, a strongly Tory district. Through material gained from observation, interviews, and questionnaires, which he subjects to elaborate quantitative analysis, the author attempts to compare the effect of differing socioeconomic patterns and political environments on the organization, leadership, and activities of the three groups.

Whatever their value to political scientists, the central findings of this study are of limited significance for historians. Although the functioning of the organizations is examined in exhaustive detail, the work adds little of importance to the admirable account of grass-roots Labour politics in Robert T. McKenzie's *British Political Parties* (1963). Furthermore, the differences that emerge among the three groups are largely predictable. It is not surprising, for example, that apathy and an aging leadership are the chief problems in a safe constituency, that political enthusiasm is easiest to evoke in a marginal area, or that recruitment of members posed difficulties for a Labour party operating in overwhelmingly Conservative territory.

The student of the political developments of the 1960s will, however, find material of interest in what the author describes as a "side-trip" (p. 281), an analysis of the opinions of party members on the political issues of those years. The assumption of Ralph Miliband and others that activists in the constituency parties were ideological enthusiasts hostile to the Labour Party's moderate leadership has recently been questioned by Richard Rose and other authorities. Turner's findings tend to support the view of the latter group. While the majority in the three organizations opposed revision of Clause IV of the Labour Party constitution, which calls for public ownership of industry, they also rejected unilateral disarmament and expressed general satisfaction with the national leadership of the party. The study also reveals a surprisingly high proportion of working-class members among the minority who supported unilateralism, a finding that calls into question the picture of the movement as a predominantly middle-class phenomenon. Recognizing that his sample of one hundred and forty-two party activists was small and geographically skewed, the author is modest concerning the significance of these results. For the historian of the period, however, they constitute the work's most useful contribution.

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ELAINE WINDRICH. *Britain and the Politics of Rhodesian Independence.* New York: Africana Publishing. 1978. Pp. 283. $26.00.

Rhodesia-Zimbabwe constitutes one of the most intractable cases of decolonization. How did Ian Smith and his landlocked state declare U.D.I. and survive sanctions for over thirteen years? Why did British politicians, Labour and Conservative alike, allow the recalcitrant white minority to avoid decolonization consistent with the rest of the empire? These questions have been asked by the former American ambassador to Zambia, Robert Good, in *U.D.I.: The International Politics of the Rhodesian Rebellion* (1973) and in the latter chapters of *A History of Rhodesia* (1978) by Lord Robert Blake. Elaine Windrich, a former Labour Party colonial affairs researcher, approaches these questions from the perspective of a detached insider.

This narrative commences in the early 1960s, covers U.D.I., the *Tiger* and *Fearless* talks, the Home-Smith agreements and the Pearce Commission. It closes in 1977 as Secretary of State Kissinger's African shuttle diplomacy led to U.N. Ambassador Andrew Young's efforts in behalf of the Anglo-American settlement proposals.

Since 1923 Rhodesia has been virtually a self-governing colony. Britain had ultimate responsibility but little internal influence. When the problem of replacing the white regime with African majority rule became paramount in the 1960s Britain was politically, psychologically, and financially, unprepared. Rhodesian-born Ian Smith perceived Brit-

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ain's lack of will although he did not judge the African majority accurately. Smith extracted ever-more-favorable settlement terms from ever-more-pliant British statesmen. In 1974 the Portuguese coup and withdrawal from Mozambique and Angola deflected Rhodesia from emulating South Africa's apartheid model. The regional struggle became internationalized.

Windrich states in her preface that "Although I have had access to confidential party and government documents, I have not made use of any information that is not already available for publication." This is unfortunate as potentially important questions may go unanswered or unasked. Why did Mr. Wilson renounce the use of force? Windrich indicates the reason "has yet to be established" (p. 58). "Oilgate," implications of official complicity in sanctions-breaking that allowed Rhodesia to obtain desperately needed oil, is not covered. More complete explanations of such events must await the documents' emergence into the public domain.

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ALEXANDER FENTON. *The Northern Isles: Orkney and Shetland.* Edinburgh: John Donald Publishers; distributed by Humanities Press, Atlantic Highlands, N.J. 1978. Pp. x, 721. $30.00.

This carefully planned book represents approximately twenty years of work on the Northern Isles by Alexander Fenton, curator of the country life section of the National Museum of Antiquities of Scotland. Fenton describes his book as a "contribution to the subject that is now commonly known as European ethnology" (p. 624).

Fenton has chosen to work on a miniature, to etch in each detail, but, unlike the standard comparative ethnological study of static, isolated primitive tribes, he carries this small society and its cultural and economic relations with other societies through 1,000 years of history. This historical ap proach to ethnology is, one takes it, what Fenton means by European ethnology.

The evidence Fenton uses in this work is impressive: Pict and Viking archeological remains at Jarlshof in Shetland and Birsay in Orkney; fifteenth-, sixteenth-, and seventeenth-century barony court books and church session records, stewards' accounts, diaries, business records, artifacts of all types, comparative language analysis, and oral history. Since the machinery of footnotes, bibliography, and index are well orchestrated, the reader can easily follow Fenton's scrupulous use of the many different sources required for this type of historical and anthropological study.

The Orkney and Shetland Islands stretch in a line from the northern tip of Scotland into the North Sea and for a thousand years have been the area in which the Scandinavian and Celtic peoples have mixed. With the total combined area of the islands only 920 square miles, population pressure exacerbated land scarcity. A straight line rise in population took place from 1800 in both island groups to a combined figure of 64,000 between 1845 and 1870, followed by a straight line decline from 1880 to 1965 when the combined population was 37,000. Lastly, sea distances taken from Lerwick in the Shetlands tell much about the islands' external relationships: to the nearest port on the Norwegian coast just west of Bergen 250 miles, to Aberdeen 200 miles, to Newcastle 378 miles.

The land and the sea, farming and fishing, and what men and women could take as a living from these two primal sources is the story Fenton tells. In careful detail the author explains how these people worked, divided, misused, and improved their land. Simple endurance in the face of inhospitable climate and limited resources was not enough; ingenuity and adaptability were the keys to survival.

The book abounds in excellent diagrams, illustrations, and architectural designs that show how these people made tools, houses, mills, and boats. His explanation of the manner in which grains of various types were grown, harvested, stored, and ground is a model of clear exposition of processes no longer in the experience of modern urban men or even modern industrial farmers (pp. 333–410).

If one remembers that prior to the nineteenth century the major commitment of European manpower was to farming and in coastal areas to a combined farming-fishing economy, the value of this book to European historians is evident. Just as they are about to disappear, Fenton has captured in this conservative community of Islemen farming and fishing skills and techniques that can be traced back through present practice and living memory to the Norse sagas and archeological remains. To the European historian puzzling over a seventeenth-century will or court case, the importance of the division of straw may seem to involve only thatching and bedding for cattle and humans. After reading Fenton's section on ropemaking from straw and bent grass, one understands more clearly the arguments and bloodshed over the disposition of this precious commodity (pp. 184–87, 264–69).

For the historian of the modern period, this book also provides some fine examples of how the demands of an industrialized urban society affected a heretofore self-contained fishing and farming economy: kelp burning from 1750 to 1825 to provide alkali for Newcastle crown glass, rapid enclosure after the 1840s for cash crops and for wool, or changes in the local fishing industry from 1790 to 1960 tied to

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the palate of an expanding British and Continental population.

Fenton indicates the one weakness of this book, that it is concerned primarily with how men gained their living in the Northern Isles rather than the men and women and their lives and aspirations. One hopes that Fenton will do the companion work on the people of the Isles. If he offers us this latter work, little will remain to be written about these hardy, adaptable, and ingenious people.

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A. P. W. MALCOMSON. *John Foster: The Politics of the Anglo-Irish Ascendancy.* New York: Oxford University Press, for Institute of Irish Studies, Queen's University of Belfast. 1978. Pp. xxviii, 504. $41.00.

This public life of John Foster is an important addition to our knowledge and understanding of the Anglo-Irish Protestant Ascendancy in the late eighteenth century—its golden age—and the very early nineteenth century. The author defines the Ascendancy as the descendants of people of English origin settled in Ireland during and since the fifteenth century, of the upper classes and Anglican in religion. By the eighteenth century they had been joined by those families of Norman and Gaelic descent who had become Anglicans.

Born in 1740, John Foster was a member of the Irish, and after 1800 the British, House of Commons for a continuous sixty years. For almost the whole of the period 1777–1811 he held government office, being the last speaker of the Irish Commons (the speakership counting as a government post). He regarded the Ascendancy, to which his family belonged, as the Irish nation, and he labored all his life to protect and perpetuate that very limited nation. Seeing the Ascendancy and the British Government as interdependent, he was a firm upholder of the British connection. Since he considered the Irish Parliament a bulwark of the Ascendancy, he led the opposition to the Union in 1799–1800, thereby injuring his own political position since his stand antagonized Pitt and his cabinet. Once the Union was effected, however, he became a sturdy Unionist since he continued to see the British government as necessary to the survival of the Ascendancy.

Having inherited an encumbered estate of some six thousand acres of good land, Foster was never out of serious debt, but he was an improving landlord and was concerned for the welfare of his tenants. Honest, intelligent, and hardworking, he was particularly interested in economics and finance, and his country benefited from that interest. He saw material prosperity as the cure for discontent, whether of middle-class Protestants seeking parliamentary reform or of Catholics seeking emancipation. He opposed both efforts because of their threat to the Protestant Ascendancy.

This book is no mere "life and times" since A. P. W. Malcomson furnishes much new information and thoughtful comment on political patronage and "jobbery" at both central and local levels of government, the exercise of the franchise in parliamentary elections, local politics as pursued by office holders and landlords, and English-Irish relations at viceregal and British cabinet level. The author uses Foster and his native county, Louth, and the borough in which he had a family interest, Drogheda, as the pivots on which his book turns, but he makes worthwhile comparisons with other officeholders and landlords and other counties and boroughs.

Malcomson rescues the Ascendancy from some of the opprobrium that has traditionally been cast upon it. For example he shows that the charge that it was corrupt and addicted to jobbery is exaggerated. This charge emanated from British statesmen who had no right to point the finger of scorn since they themselves dispensed much Irish patronage to Englishmen who had done nothing to earn it.

The publishers are to be congratulated on placing footnotes at the bottom of each page, a practice growing rare as printing costs rise. The book has one defect in that the author does not point up his major conclusions. Thus the reader must either note down those conclusions as he reads or go back over the book to refresh his memory. It is surprising to find such a blemish in a book that in every other way maintains a high level of scholarship.

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RUTH DUDLEY EDWARDS. *Patrick Pearse: The Triumph of Failure.* New York: Taplinger Publishing. 1978. Pp. xv, 384. $14.95.

Partisanship rather than analysis has been the hallmark of most writing on the founders of the Irish state. Ruth Dudley Edward's balanced and perceptive book is on a different level. Using much new material, she offers important insights into Patrick Pearse's career, set in the general context of cultural and revolutionary nationalism in Dublin, from 1900 to 1916.

Pearse was the son of an English free-thinking father (whose career as a church sculptor required nominal conversion to Catholicism) and a devoutly Catholic Irish mother with whom he lived throughout his life. Many of his everyday attitudes were snobbish yet social aspiration in him was more than mere social conditioning. Ambition and impatience, driven by a restless energy, were the keynotes of his career, moderated by a capacity to learn from expe-

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rience that carried him slowly and painfully beyond his "restrictive education and stultifying home life" (p. 93). Thus the young teacher of Irish whose denigration of the English language drove the student James Joyce to take up Norwegian in order to escape from such a bore and who would have preferred to see W. B. Yeat's (English-language) Irish Literary Theatre "strangled at birth" (p. 31) was able later to come to a more cosmopolitan appreciation. His total ignorance of the lives of Dublin slumdwellers was tempered somewhat after his later association with James Connolly. On the other hand Pearse held throughout his life a view of Ireland's problems so crude as to make him regard emigrants as traitors and fools, to denounce those who accepted old age pensions from the United Kingdom government for selling out to the enemy, and to maintain that the Irish population under "free conditions" could expand within a century to twenty millions on the basis of agriculture and rural industries, with "no Glasgows or Pittsburghs."

Pearse's work for the Gaelic movement was valuable. His literary work revealed at least a modest talent (notwithstanding the embarrassing and unconsciously pederastic tone of items like "Little Lad of the Tricks," which enhanced his reputation neither as a poet nor as an educator of young people). His initial success in attracting the children of the intellectual elite to his school attests to his appeal as an educational innovator as well as to dissatisfaction with priest-dominated and examination-fixated Catholic education. But it is Pearse's brief career as a revolutionary that justifies a full bi ography. Here Edwards's interpretation is clear and convincing. Pearse's fatal flaws of personal vanity and ambition, coupled with his near-criminal disregard for financial planning in journalism and school administration, had brought his personal career to the brink of disaster by 1912. This failure set him on the road to revolution. Psychologically it created a frame of mind in which he could "bear to see it [his school] go down gloriously by my imprisonment for a political offence," but not to see it collapse "squalidly," that is, financially (p. 239). More specfically, it caused him to plan an American tour in search of new funds, which led him into the arms of the Irish Republican Brotherhood and the Irish-American sponsors of rebellion. His "stately bearing" and his ability to inspire in speech and print made him the natural figurehead for the rebellion that followed at Easter 1916. More than that he never was, unless it be said that his heady and blasphemous rhetoric of blood sacrifice swayed saner men to the view that a self-destructive rising was the necessary beginning to a campaign for secession from the UK.

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DAVID W. MILLER. *Queen's Rebels: Ulster Loyalism in Historical Perspective.* New York: Barnes and Noble. 1978. Pp. xiii, 194. $22.00.

Extended essays—as distinct from conventional monographs—are not much in favor in North American historical writing. The cogitative, reflective, and often elegantly argumentative qualities of the historical essayist seem not to fit with our present culture. To his credit, David W. Miller has swum against the tide and produced a remarkably stimulating essay upon loyalism in Ireland. Certainly no county in the Western world has a history more demanding of reflection. In the twisting skein of Irish history, getting the story straight is the easiest (but by no means easy) part of the scholar's job; deciding what the story means is the hard part.

The great virtue of Miller's essay is that one can carry on a running conversation with the author, the one minute struck with the saliency of an insight, the next engaged in composing an answer to a point that seems not only wrong but downright wrongheaded. That is the response a successful essay produces.

Miller knows his Ulster and has pondered well upon it. His key point is that loyalism in Northern Ireland has the character of "conditional loyalty": it is granted to the crown under a set of unstated covenants that justify the loyalists (in their own eyes) in being on occasion treasonable to the state. Miller defines this conditional loyalty in terms of a contractarian genealogy that has its roots in seventeenth-century Scottish and English thought. He argues that the character of Northern Ireland's industrialization served to reinforce the contractarian character of the Ulster Protestants.

The essay's argument is rich and cannot be detailed or fully criticized in a short review. In my judgment, Miller is most convincing when he is putting forward his own observations. When he borrows his concepts from others, as occurs with his use of Gellner's modernization theory, his essay loses its pungency and becomes merely derivative. And at times his argument becomes so abstract that the tone ceases to be historical and becomes almost theological. Fortunately, however, originality and crisp common sense characterize most of the work. Irish historians will find few facts in the book that they do not know already, but they can scarcely fail to think afresh about what they thought they already knew.

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MAURICE LEVY-LEBOYER, editor. *La position internationale de la France: Aspects économiques et financiers, XIX^e–XX^e*

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siècles. (Association française des historiens économistes, Deuxième congrès national, 5–6 octobre 1973). Paris: École des Hautes Études en Sciences Sociales; distributed by Librairie Jean Touzot. 1977. Pp. 472.

This book is a collection of papers that were prepared for and presented at the second *Congrès de l'Association française des Historiens économistes*, which concerned itself with the problem of the international position of France in the nineteenth and twentieth centuries. The *Congrès* addressed itself to the broadest possible number of economic and historical topics, and most of the participants presented work they had begun as students of the doyen of French historians, Pierre Renouvin. One of the major themes examined at the *Congrès* was the exportation of French capital at the time of the First World War. This particular chronological period was chosen because it was felt that it is susceptible to interpretations that might be applied profitably to other periods.

The idea has long been prevalent that France, much more than other countries, accorded too high a priority to overseas investment and in doing so squandered its resources and acted in ways that were detrimental to its basic economic interests. Recently, however, a number of scholars have found this idea at variance with some basic principles of quantitative history. The old view was that France, because of the growing disequilibrium of its foreign exchange, the low level of revenue, and a decline in its exportable savings, was forced at the beginning of the twentieth century to accelerate the importation of foreign capital and to copy and utilize foreign technology. More recent studies appear to indicate that too much weight has been given to the importance of foreign investments and that these earlier conclusions should now be revised. France had long been accumulating capital, but was its exportation of this capital concentrated in the period of active industrialization just prior to the First World War, and therefore injurious, or was it a movement spread out over a period of time that favored economic expansion? The articles published here address this problem. In an attempt to have a balanced presentation, the articles are classified under six major headings: the historical data concerning the commercial position and financial capacity of France; the principal experiences of direct investment and financial investment that followed the nineteenth century; the role of political factors; the war and depression of the 1930s; the new forms of investment in the interwar years and after 1945; and finally the general perspectives that bear on the different points of social history, Marxist analysis, and economic observation.

For the historian, sections two and three are probably the most significant. Eight very interesting articles examine the political factors involved in French investment in various European countries—Germany, Austria, Hungary, Italy, Russia, and the Ottoman Empire. In section four there are two particularly good pieces. A. S. Milward, in his article on the investments of the French in foreign countries and the two world wars, attempts to answer the question "what proportion of the French foreign investments was lost as a result of the two World Wars?" D. Artaud's article on the "French War Debts 1919–1929" is also worthy of note.

The articles are well balanced, but this work will appeal primarily to the specialist who is interested in French foreign investments in the late nineteenth and early twentieth centuries.

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RICHARD COBB. *Death in Paris, 1795–1801.* New York: Oxford University Press. 1978. Pp. 134. $12.95.

Recently French historians have been courting death: three important books have appeared, Philippe Ariès' *L'homme devant la mort* (1977), Pierre Chaunu's *La mort à Paris, 16ème, 17ème, 18ème siècles* (1978), and now Richard Cobb's *Death in Paris, 1795–1801.* This little book represents a tour de force based on the contents of one box of documents in the Archives de la Seine. These documents record 404 violent deaths, two-thirds of them suicides. Almost all of the corpses were eventually identified by up to four witnesses. The defunct, their clothing, the witnesses, and occasional remarks by the two "concierges" at the morgue, bizarrely named Daude and Bouille, provide hundreds of tiny clues to the social historian. Cobb uses these to reconstruct the existence of the victims just prior to suicide or murder: it is thus not death that is chronicled here but the collective life of extremely poor Parisians in central Paris, near the Seine river, where nine-tenths of the suicide victims sought death by drowning.

Only Cobb's deep sense of humane sympathy, his intimacy with the sights, sounds, smells, and rabbit-warren geography of revolutionary Paris could resuscitate these dreary ghosts. Cobb reconstructs a "network of neighborhood, work, and leisure" owing to his unique blend of dedicated sleuthing, inspired hunches, imaginative inference, and obvious bias against the authorities. Biography is here written backwards from a corpse to the last moments of life.

Corpses abounded in clues. The poor usually wore all their clothing and carried all their possessions since their rooms lacked storage space and since people on night shift often slept in their beds

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during the day. "In the world of the poor, relatives, *intimes*, and neighbors would soon find their bearings in an intimate and unique geography of patches, mendings, tears, odd buttons, odd shoes, tunics, aprons, stitchings and seams" (p. 21). Cobb concludes that the suicide victims were not lonely but "could have had someone to turn to, at the time of the ultimate crisis" (p. 61). The record suggests social immobility: relatives often worked in the same menial trade and lived in the same neighborhood. Cobb concludes that "women rarely commit suicide in winter"; and "the likeliest choice for suicide would be a Sunday, Monday, or Friday, in the first half of May" (p. 8). Spring is generally recognized as suicide season.

This book includes copious verbatim testimony. Footnotes and appendixes make up almost half the text. The specialist in French history will be richly rewarded but only francophones should approach it. Most of the documentation is in French, and phrases abound that could easily have been translated, such as "half a dozen *voisins de palier*," "her ultimate decision *d'en finir*," "the much feared *dangers de la ville*." The most frequently quoted author is Restif de la Bretonne. No mention is made of the authorities' attempts to rescue drowning persons, and yet the records of the *Conseil de salubrité de Paris* at the *Préfecture de Police*—though extant since 1801 only—indicate that first-aid boxes, buoys, and gear to aid resuscitation had long been placed at strategic points along the river. The rescued, of course, would not reach the morgue and thus do not swell Cobb's macabre evidence.

This is not a sad book. Cobb finds one hundred and fifty suicides a year a reasonable level for a city of 700,000 persons. He points with amazement to the virtual absence of murder (twenty a year, at most). He perceives the poor districts of revolutionary Paris as "a world of neighborliness, of watchfulness, of enforced 'living together', which, however crude and brutal, noisy and filthy, quarrelsome and envious, was not devoid of compassion, tenderness, kindliness, and disinterestedness." That is a generous interpretation of the evidence.

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NORMAN HAMPSON. *Danton.* New York: Holmes and Meier. 1978. Pp. x, 182. $19.75.

"It might be right to reject everything that can't be positively proved, if you were trying him for murder, but unless I'm mistaken, your verdict of history is always going to be 'not proven,' " observes one of the narrators in Norman Hampson's *The Life and Opinions of Maximilien Robespierre* (1974). The same remark applies to his biography of Georges Jacques Danton, which is a natural complement to his life of the Incorruptible. The author has wisely abandoned the technique of using multiple narrators to express ambivalence about his subject, but he remains no less cautious about rendering any strong judgments.

From the outset Hampson offers as his excuse that reliable information about the orator-statesman's career is lacking. Danton's own evasiveness compounds the difficulty, for as the author notes: "His speeches were . . . often masterpieces of ambiguity which may or may not have meant the opposite of what they purported to say" (p. vii). But the theme of "we are seldom sure of what he did say and whether or not he meant it" (p. viii) that runs through the book only weakens the author's effort to write a penetrating biography. Unable to decide whether Danton was a "generous and open-hearted *bon viveur* or a cynical crook" (p. viii), Hampson prefers to weigh the conflicting sources for his subject's life, rarely entering the man's mind or clarifying his motives.

True, the author does insist that Danton was a "practical man who could speak the language of total revolution and perhaps even carry himself away by his own rhetoric, but he never shared the millenarian views of those intent on creating a new republican man" (p. viii). Danton is depicted as a practical politician, a realist and pragmatist, and, most important, a "professional politician of a new type, who combined a cool head for tactics with the kind of extrovert ebullience that won sympathy if not respect" (p. 66). Unfortunately, because his political aims are never fully explained, Danton emerges simply as an opportunist who seemed to have no fixed principles, who maneuvered for advantage from all sides, and who sought office only to benefit himself and his cronies. If Danton was indeed the skillful political operator that Hampson believes, why did he fail so frequently to win office or to maintain himself in power for very long? What accounts for Danton's failure to display vigor at such critical moments as the overthrow of Louis XVI on August 10, 1792? How could he have allowed himself to be so easily toppled in March 1794? Leaving these questions unanswered, Hampson concludes the biography abruptly with Danton's injuncture to his executioner to "show my head to the people; it's worth seeing" (p. 174). The disappointed reader closes the book wondering why.

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R. B. ROSE. *Gracchus Babeuf: The First Revolutionary Communist.* Stanford: Stanford University Press. 1978. Pp. viii, 434. $18.50.

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In this lucid, balanced account, R. B. Rose, an Australian noted for his earlier judicious treatment of five *Enragés*, interweaves trenchant analysis with gripping narrative to unfold the diverse, sometimes apparently contradictory, activities of François-Noel Babeuf (1760–97). Here fundamental new insights into the character of perhaps the most controversial Revolutionary publicist are presented in contrast to the perspectives of abundant recent works in Russian, Italian, and German, as well as French.

Rose acknowledges a fascination with ideas and traditions instrumental in the growth of "political freedom and social equality in the modern world" (p. v). Denied access to manuscripts in his hero's hand at the Marxist-Leninist Institute, Rose conscientiously consulted copies and analyses of many of these as well as originals of those available in Amsterdam, Paris, and Amiens and the enormous corpus of printed works by and concerning Babeuf. He considers himself a political biographer because evidence regarding his subject's private life is scanty. Rose concedes that, to achieve coherence, he schematizes ideas Babeuf conceived in disparate circumstances. Nevertheless he strives to evoke the original context of every action, thereby avoiding the pattern that, since the eighteenth century, has mythicized Babeuf's thought for praise or condemnation He leaves judgment of much evidence to the reader but offers sensitive and generally plausible analysis of motives. Unfortunately, the publisher's relegation of notes to the end of the volume renders the argument less compelling. The author employs a few seductively simple categories such as "royalist" and "bourgeois" without confronting their ambiguity, but his referents are generally clear. He occasionally lapses into a vocabulary suggesting a belief that the genealogy of an idea can be traced from one thinker to another without regard for the climate of opinion. Yet Rose realistically examines Babeuf's halting progression from belief in direct democracy to a program of revolutionary communism that advocated equality for women as well as collective ownership and use of resources. He points out that numerous writers of whom Babeuf was scarcely aware proposed social models implying change at least as radical as the visions of moral regeneration that Babeuf put in writing before the Revolution.

Emphasizing the profound tensions between ideas and practical concerns, Rose employs this multifaceted life to illuminate a variety of social milieux and many peculiarities in an era of general crisis. The son of a poor tax administrator in Picardy, Babeuf labored there as an ordinary canal digger before learning as apprentice how to advise seigneurs seeking to maximize payments by peasants. He proposed innovative techniques that improved efficiency of exploitation for his employers. But from about 1786 his lack of deference to social superiors sometimes reduced him, his wife, and his children to dependence on friends' charity. He began to record, though not yet to publish, ideals generated by a fertile imagination. These challenged the very order in which he had aspired to a profession. He hypothesized that all existing property originated in theft and fraud during an era of feudalism, that all other rights were subordinate to the exigencies of survival, and that it was just to seize the means necessary for subsistence. From 1789 he embraced popular revolution and served the department of the Somme and the commune of Paris as an official. However, magistrates of the constitutional monarchy and various republican regimes incarcerated him, repeatedly and for long periods, before the Directory finally guillotined him.

Experiences under several tyrannical regimes—Jacobin Revolutionary Government, the Thermidorian Convention, and the Directory—prompted Babeuf, like many alert fellow citizens, to abandon aspirations for unanimity in favor of competition among elite parties. Obsessed with the goal of absolute equality, he advocated a brief proletarian dictatorship to achieve it. In dark moods, he considered mankind doomed to perpetual civil war between the governed and rulers, who could be only partly constrained by popular intervention to redistribute property. Even when most optimistic, Babeuf assumed that the masses would remain impoverished. Unable to conceive the growth of mammoth factories, he presumed a relatively static economic order. After prolonged isolation in prison, presumably pressed by the logic of his own messianism, he plotted a coup to achieve sudden social revolution. In retrospect, he seems to have anticipated the neo-Jacobinism of Blanqui or even the utopian anarchism of Proudhon more clearly than the dynamism of Marx's dialectical materialism, let alone Lenin's theories of political leadership.

JAMES N. HOOD
Tulane University

GWYNNE LEWIS. *The Second Vendée: The Continuity of Counter-revolution in the Department of the Gard, 1789–1815*. Oxford: Clarendon Press. 1978. Pp. x, 250. $26.00.

With some of its roots embedded in the soil of Reformation France, and its branches showing remarkable sturdiness for generations after the restoration, the counterrevolution in southeastern France reached frequent peaks of power over a period of twenty-five years but did not achieve its political goal of federalism or separatism, even though the long-term results of royalism erected a formidable

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barrier to republicanism for much of the nineteenth century. Gwynne Lewis's original study of the Gard is a noteworthy contribution to our perception of the resistance to the revolution and reveals how that region was, if not altogether different from the better-known Vendée of western France, neither its carbon copy.

Counterrevolutionary activity in the Gard, which expressed in large part the same kinds of discontent throughout lower Languedoc, erupted into violence during three major periods—from 1789 to 1792, intermittently from 1795 to 1799, and again with new vigor after 1808, but with explosive force in 1815. Although Lewis does not neglect the relationship between Reformation and revolutionary patterns of religious massacre, he gives their psychosociological foundations less weight than they warrant. This does not, however, detract from a subtle analysis of the sources of socioeconomic rivalry, which manifested itself in the first phase of the revolution in the political ascendancy of the Protestant *négociants, financiers*, and *marchands fabricants*, whose wealth in textiles (mainly silk) began to overwhelm not only Catholic landowners, *officiers*, and members of the legal profession but also to intimidate and embitter the less affluent Catholic merchants. The radicalization of the revolution produced an unexpected alliance between the rival elites who had become the chief targets of popular rule and terror, inducing in these erstwhile enemies a common front based on class that gave fresh support to federalism and prepared the ground for the drastic purges of the radicals between 1795 and 1797. If, however, this constituted a class conflict, it was, Lewis adds, more of a struggle between "les petits contre les gros" (p. 140). The argument that the *ouvriers* never dreamt of challenging "the profit motive or the theory of surplus value" is nevertheless somewhat anachronistic (p. 141). Lewis is not always certain that these are the appropriate categories (see pp. 52–54), since he seems also to be saying that periodic economic crises in the textile trade and the attempts to reduce urban *ouvriers* to the same status as the rural proletariat sharpened friction. The economic crisis of the last years of the Empire revived nearly all, if not all, of the same configurations as the earlier ones, including the rhetoric of nostalgia, religious hatred, the recruitment of poor Catholics, and plans to vest power in separate provincial administrations.

Lewis's treatment will be read with respect because he does not turn away from the complexities of these problems. Nor is he averse to ascribing the royalist massacres to the calculations of a number of key individuals whose names provide the connecting links from one explosive episode to another: the simple spontaneous outbursts are a myth exploited by those who would thus excuse them as demonstrations of uncontrollable passion. There will be readers who will expect more exhaustive information on land tenure, the textile industry, and income levels. Its absence is not a serious shortcoming. He has captured much of the febrile atmosphere of the Second Vendée and writes clearly, if at times rather tantalizingly, about it. One of its ironies is only indirectly revealed. The violence of the elites not only matched the violence of the poor but also was older and more deliberate.

HARVEY MITCHELL
University of British Columbia

LOUIS BERGERON. *Banquiers, négociants et manufacturiers parisiens du Directoire à l'Empire.* (Ecole des Hautes École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques. Civilisations et Sociétés, number 51.) The Hague: Mouton; distributed by Walter de Gruyter, New York. 1978. Pp. 436.

The economic history of the First French Empire has been neglected and too readily dismissed as a disaster for the economic development of the Continent. Without denying the special conditions imposed by constant warfare, the English blockade, and the Napoleonic counterblockade, Louis Bergeron shifts economic attention from the decaying port cities to the dynamism of Paris, "her back to the sea," launching France and much of Western Europe on its first *démarrage* into industrialization. Even more surprising to those who have read David Landes and François Crouzet is Bergeron's stress on the innovative role of Parisian bankers and entrepreneurs under most difficult circumstances. Nor were all the bankers and *négociants* Protestants, Jews, and foreigners, the view so dear to Max Weber and Werner Sombart. Good provincial Catholics held their own, along with, to be sure, those networks of Huguenot and Jewish families described by Louis Dermigny and Herbert Lüthy, Jean Bouchary, and Paul Butel. "The Napoleonic period is surely one where the will to change the balance of economic power of the Continent to the detriment of England set off an irreversible process of modernization" (p. 319).

In addition to this touch of "voluntarism," unexpected from an associate of the *Annales* school, Bergeron also makes place for the "event" in economic history. He is describing, after all, flesh-and-blood entrepreneurs who faced changes in market demand and in the supply of capital and raw materials; events and individual decisions do count.

The author's thorough examination of business letters and inventories, partnership agreements, and bankruptcy statements qualifies or refutes a number of older generalizations about French economic be-

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havior. The passion for landholding was not necessarily a brake on economic growth; land was indispensable collateral for raising commercial and industrial capital. Buying land and *rentes*, even entering the army or politics, did not preclude being a "brasseur d'affaires." "One must not attach too much importance to internal cleavages among the bourgeoisie rentière, bourgeoisie d'offices, bourgeoisie de la marchandise et de l'argent" (p. 40). Nor do Parisian credit and banking techniques appear as conservative as the older textbooks would have us believe. If the Bank of France under Bonaparte restricted credit operations to short-term loans and refrained from becoming a bank of issue, private Parisian banking houses were numerous and willing to risk capital on new commercial and industrial enterprises, especially in cotton textiles, coal, metallurgy, and the new chemical industry. One of the best chapters in this carefully researched volume treats the Oberkampf cotton enterprise, demonstrating that entrepreneur's remarkable resilience and drive and also confirming some of the older generalizations about French merchant-manufacturers, their meticulous concern for quality goods regardless of price, a tendency to expect equipment to last forever, and a policy of financing new investment out of profits or by loans from a restricted circle of close friends and relations.

Quite consciously, Bergeron has written neither an *histoire totale* nor an essay on the "new economic history." This is a business history that shows how institutions function and how entrepreneurs act. By French standards it is a monument of brevity and precision—only four hundred pages including tables, maps, notes, and bibliography. It is a model monograph.

ROBERT FORSTER
Johns Hopkins University

EMMET KENNEDY. *A* Philosophe *in the Age of Revolution: Destutt de Tracy and the Origins of "Ideology."* (Memoirs of the American Philosophical Society, number 129.) Philadelphia: The Society. 1978. Pp. xviii, 382. $12.00.

The past century-and-a-half has not been kind to the reputation of Antoine Destutt de Tracy (1754–1836), a leader of that offshoot of the French Enlightenment that he was the first to call Ideology. Responsibility for this historical neglect can be placed in part upon the derivative nature of the movement: it forswore innovative brilliance and became primarily a systematic elaboration of a "sensationalist" moral and political system that already seemed somewhat barren and dated by the early nineteenth century. Moreover, the republican, libertarian principles of Tracy and his colleagues earned the enmity and scorn of Napoleon Bonaparte and hence of official France, while their predominantly cool intellectuality seemed thin blooded to the enthusiasts of the rising romantic movement. Nor did Ideology in its original form show any real recuperative strength after Bonapartism and romanticism had passed their crests. Mid-century liberalism found Ideology too narrow and dogmatic; positivism declared it too metaphysical and individualistic, Marxism ridiculed its ties to bourgeois capitalism.

All these are among the facts persuasively presented in Emmet Kennedy's *Destutt de Tracy and the Origins of Ideology*—a study notable, among other attractive qualities, for not claiming too much for its subject. The main emphases of the work are the life, career, and philosophical development of Tracy himself. In the absence of any other full-length, serious, comprehensive, and modern study of Tracy, Kennedy's book fills a significant historiographical gap. Thus, it takes a place beside highly specialized monographs (notably François Rastier's formidable *Idéologie et théorie des signes: Analyse structurale des éléments d'idéologie d'Antoine-Louis-Claude Destutt de Tracy* [1972]), general studies of early Ideology (including Emile Cailliet's admirable *La Tradition littéraire des idéologues* [1943]), and several studies of Tracy's fellow *idéologues*, especially Maine de Biran.

Kennedy's biographical information is more than ample. With few exceptions the biographical sections avoid psychological speculation and are factually straightforward. If one sometimes regrets the lack of vivifying insights into personality, the defect may well be more in Tracy's character than in his biographer's skill. Tracy's "science of ideas" is developed within a biographical context rather than as an isolated philosophical system. Ultimately, nevertheless, a cogent presentation of Tracy's thought does emerge, with analyses of its tendencies toward measurement and quantification, its intellectual reductionism, and its dogmatic claims to philosophical comprehensiveness and logical infallibility. The thoroughness of Kennedy's research is reflected in an extensive bibliography, and the book's value is augmented by an exceptionally full index. The author's vision of the Enlightenment does appear a bit simplistic: for example, its classicism was not as narrow or its philosophical approach as static and nonrelativistic as he implies (pp. 333–34). These minor flaws do not, however, seriously mar a substantial and useful contribution to the history of ideas.

HENRY VYVERBERG
Southern Illinois University, Carbondale

MAURICE CROSLAND. *Gay-Lussac: Scientist and Bourgeois.* New York: Cambridge University Press. 1978. Pp. xvi, 333. $36.00.

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Maurice Crosland's biography of Joseph Louis Gay-Lussac (1778–1850) is no disappointment to the reader acquainted with Crosland's distinguished studies of the language of chemistry, the Society of Arcueil, and the traditions and institutions of French science. This newest book is a fine contribution to the biographical and historical literature of science. Crosland has constructed a thematic biography rather than a chronological one, and while there is occasional repetition and untidiness, the soundness of presentation and a useful chronological table ensure that Crosland's result is an admirable one. Beyond its service as a biography of a great chemist, this monograph is a vehicle for studying some principal features of French science from the *ancien régime* to the eve of the Second Empire.

Nowadays schoolchildren learn Gay-Lussac's name in elementary chemistry textbooks when they are introduced to the chemical combination of gas volumes in ratios of small whole numbers. Historians of science have remarked the seeming paradox that this law of Gay-Lussac appears in modern chemistry textbooks as a proof for Dalton's atomic theory, yet Dalton rejected the law, and Gay-Lussac failed to employ Dalton's theory. Crosland skillfully explains these apparent anomalies, detailing lucidly and elegantly the simultaneous use in early nineteenth-century chemistry of notions and notations of atomic weights, equivalent weights, combining volumes, and gas or vapor densities. With an eye to the influences of Lavoisier, Berthollet, and Laplace, Crosland ably interprets Gay-Lussac's choice of research problems, his quest for physical and chemical laws, and his views on proper scientific method. In addition, Crosland assesses Gay-Lussac's aims and achievements in relation to the contemporary research of Dalton, Wollaston, Davy, and Berzelius, sketching in particular an arch rivalry with Davy.

Gay-Lussac's initial studies of gases and their volumes won him a seat in the Institute's section of physics. Often collaborating with Thenard, his studies and preparations of potassium, boron, chlorine, iodine, and cyanogen contributed substantially to our knowledge of chemical elements and composition, including the problem of the nature of acids. Pure and applied chemistry intermingled in Gay-Lussac's work as both government and industry in France perceived the practical value of the new chemical expertise. We learn that Gay-Lussac was consultant to the Saint-Gobain glassworks and director of the Assay Bureau at the Mint. When asked by the French Government for advice on determining alcoholic content of taxable brandy and spirits, Gay-Lussac designed an alcoholometer in "degrees Gay-Lussac" that was marketed by his business partner to the government at five francs apiece.

As the title of Crosland's book is meant to emphasize, Gay-Lussac was both scientist and bourgeois. Crosland argues that Gay-Lussac was one of a first generation of professional, middle-class scientists educated through specialized scientific training. Motivated in part by bourgeois goals and values, he supported himself comfortably in Paris through the *cumul* system of simultaneous teaching appointments, as well as through consulting fees, instruments manufacture, patent rights, and wise investment in shares and property. With Arago he edited and owned jointly the *Annales de Chimie et de Physique.* He was elected to the Chamber of Deputies (1831) and named to the Chamber of Peers (1839). Crosland successfully interweaves these diverse threads into the fabric of Gay-Lussac's scientific career, realizing his expressed intent to relate "as far as possible the life and the work, believing as I do that every man is a child of his age" (p. xi).

In addition to the chronological table detailing major events in Gay-Lussac's life, there is an appendix of previously unpublished correspondence, name and subject indexes, and a detailed table of contents. The bibliography is useful, though not exhaustive with respect to secondary literature.

MARY JO NYE
University of Oklahoma

EVELYN BERNETTE ACKERMAN. *Village on the Seine: Tradition and Change in Bonnières, 1815–1914.* Ithaca, N.Y.: Cornell University Press. 1978. Pp. 185. $12.50.

This slim volume complements earlier histories of Chanzeaux and Pont-de-Montvert, for it charts the growth of a very different village in northern France. Unlike the two southern villages, Bonnières increased its population and diversified its economy in the nineteenth century. To explain its dynamism, Evelyn Bernette Ackerman emphasizes its proximity to Paris and its access to transportation networks, especially the railroad. But she also stresses the "happy coincidence" of an ambitious young man purchasing and exploiting the largest local estate. Between 1861 and 1884, this "model farmer" set an example of land consolidation and crop experimentation, while his distillery, sugar refinery, and glue factory laid the foundation for an industrial town. Because the new factory workers arrived slowly and found steady employment without displacing natives or intermarrying, Bonnières experienced little of the social tension that accompanied rapid industrialization and immigration elsewhere.

Ackerman enhances her analysis of Bonnières's distinctive socioeconomic development by examining the subtler psychological transformations associated with modernization. Using family reconstitution, she demonstrates a drop in the age of brides and in completed family size and then concludes,

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like other demographers, that more couples were practicing contraception. Following Flandrin, she infers that people were exercising more personal choice in marriage partners, linking romantic love to marital relationships, in short, evolving a more sentimental ideal of marriage. To her credit, she warns of the risks inherent in deducing human feelings from vital records and notes contradictory evidence, such as the persistence of occupational endogamy. Ackerman also argues that the decline of commercial wet-nursing, better school attendance, and the appearance of a piano teacher indicate a more child-centered family.

Although much of the book is based on quantitative data, illustrative details always enliven the statistics. More important, specific individuals, families, and groups are given due attention. Ackerman accounts for the relaxed hold of the Church in a variety of ways, including the departure from Bonnières of a particularly devout, powerful family. After documenting the effectiveness of midwife-vaccinators she describes their local origins, familiarity with their patients, and dedication to their task and thereby makes a convincing case for their pivotal role in breaking down resistance to medical innovations.

In an afterword subtitled "Why Village History?" Ackerman answers that it "reintroduces real, identifiable common folk into history" and helps us "appreciate the human dimension" behind economic, social, and psychological trends (pp. 161–62). The afterword, parts of which have appeared in an article, is hardly necessary, for the text itself breathes life into the Bonnièrois without losing sight of the broader movements in Bonnières or the French countryside. Only folkloric evidence, as used by Weber, could have made this portrait of villagers any more vivid.

MARY LYNN MCDOUGALL
Simon Fraser University

NELLY WILSON. *Bernard-Lazare: Antisemitism and the Problem of Jewish Identity in Late Nineteenth-Century France.* New York: Cambridge University Press. 1978. Pp. x, 348. $28.50.

Bernard-Lazare is a passionate and painstakingly researched monograph written from a sensitive and analytical, if not always absolutely critical, perspective. Nelly Wilson desires to rescue Lazare from undeserved historical anonymity and to place him in the center of literary anarchism, the Dreyfusard movement, and Zionism. If her arguments are not always convincing, this first English biography of Lazare nonetheless contributes significantly to French, Jewish, and European historiography.

Wilson divides the account into three segments. "Before the Dreyfus Affair" traces Lazare's childhood, intellectual development, and transition from symbolist poet to successful journalist and literary anarchist. She analyzes the symbolist and anarchist movements skillfully and examines Lazare's mixed feelings toward his Jewishness and anti-Semitism.

Section 2, "The Dreyfus Affair," represents Wilson's most important contribution. Although the contention that "Dreyfus owed his freedom to Bernard-Lazare's convictions and commitment" (p. 180) is overstated, Wilson establishes that Lazare played a more crucial role throughout the affair than has been recognized, particularly with respect to creating an "international moral syndicate" on Dreyfus's behalf during 1898–99. Moreover, Lazare was more courageous and consistent than others who are better remembered, most notably Colonel Picquart. Lazare appreciated anti-Semitism's central place in the affair and, unlike the Dreyfus family or French Jewry, he desired to confront this issue.

In section 3, "After the Dreyfus Affair," Wilson insightfully probes Lazare's changed Jewish identity, manifested in his later scholarship on anti-Semitism, the genocidal possibilities of which he glimpsed, and in his colorful, contentious, and tragically short-lived career in the Zionist movement. She documents his unswerving passion for justice and freedom on behalf of persecuted Rumanian Jewry, the Armenians, and even French clerical teaching orders. Considerable portions here consist of occasionally masterful reconstructions of Lazare's last thoughts from fragmentary private notes and incompleted essays.

Although *Bernard-Lazare* is therefore a very welcome study, it is marred by several unfortunate features. Stylistically, the work is uneven, and one's patience is sometimes tried by long, untranslated quotations. Essential material is at times lodged in long footnotes which therefore become required reading.

More substantively, far too little is revealed of Lazare's family life and its relationship with his changing Jewish identity, while comparatively too much is made of his precise connections with diverse Parisian intellectuals. Thus it is not until page 240 that one learns of the passionate Zionism of Lazare's wife, with whom he apparently enjoyed a meaningful relationship, but little is done with this type of data.

Finally, although Wilson understands that Lazare's anarchistic temperament weighed heavily on others, he remains the unrecognized hero in nearly every encounter. Obviously it is painful for Wilson to be critical of this unique individual, but the reader is presented with enough material to reach a somewhat different judgment. In retrospect, Lazare's early ambivalence toward his Jewishness ap-

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pears to involve both self-hatred and insufficient learning, and, as Michael Marrus maintains: "For although he [Lazare] appears to have been both honest and forthright in polemics, his mercurial nature made him unfit for organized political activity of any sort" (*Midstream*, [June-July 1977] 37).

ROBERT I. WEINER
Lafayette College

ROBERT J. YOUNG. *In Command of France: French Foreign Policy and Military Planning, 1933–1940.* Cambridge: Harvard University Press. 1978. Pp. ix, 346. $17.50.

It takes courage to write a book on French policy in the 1930s. Monographs are few and the sources often scanty. A few historians, notably William Evans Scott and Judith M. Hughes, have written without the documents, but the majority of scholars, including the French, have steered clear of the subject. Robert J. Young's study is the first monograph to investigate the relationship between foreign policy and military planning. After a preliminary analysis of French resources and planning, the author describes the governments and crises of the time, focusing particularly on the Ethiopian and Rhineland crises and the Popular Front governments of 1936–37. *In Command of France* draws on a wide range of archival material—French public and private papers as well as American, British, and German files. Alas, it is a most disappointing work. There are two serious flaws. Firstly, the French army archives have not been examined in any depth, and consequently the documentation is too slight to provide the solid foundation needed for the subject. In effect, the documentation rests on the British papers and the Daladier archives. A check of chapter notes for the first five chapters revealed only 23 references to the army archives out of a total of 317 notes. The army papers at Vincennes are extensive, and a competent study of civil and military leadership should be firmly based on the army papers. The author's additional references to air force and navy files do not compensate for this fundamental disability. The documentation is also imbalanced. The British papers consulted loom almost as large in the footnotes as the Daladier papers and *Documents Diplomatiques Français*. In a monograph on Anglo-French policy this reliance on British sources would be commendable, but in a work of this kind it is out of place.

Secondly, and much more seriously, the book is spoiled by omissions and compressions. Of ten chapters in all, two are devoted to Anglo-French relations—a logical arrangement in view of the importance French leaders attached to the entente—but the coverage is thin and inadequate. The two-day Anglo-French conference of November 1937, arguably decisive for policy making in 1938, receives a mere half sentence (p. 155). After a cursory discussion of the April 1938 Anglo-French meeting there is a single phrase about "the follow-up Anglo-French conferences in London" (p. 207) and then nothing more. The November 1938 Anglo-French meeting in Paris is dismissed in one brief paragraph (p. 217). General Gamelin's visit to London on September 26, 1938, the first encounter of British and French chiefs of staff, is not noticed in the text. Thus neither the political nor the military themes receive adequate scrutiny. This curious, elliptical pattern prevails through the book. The Franco-German declaration of December 6, 1938 merits only one indirect reference—"preliminary conversations with Ribbentrop in December" (p. 216). The handling of the March 15, 1938 meeting of the *Comité permanent de la défense nationale* is quite misleading since no mention is made of the discussion of the possibility of French intervention in Spain—the subject that most preoccupied Blum and colleagues.

The failure to do justice to the subject springs essentially from the author's self-denying ordinance: "It is not a comprehensive study of France and Munich . . . Parliament, press, parties and pressure groups are effectively ignored, set aside" (p. 207). More's the pity. How can the relationship between foreign policy and military planning be analyzed without the context of parliament, press, parties, and pressure groups? The author's contention that France's leaders were firmly in control of policy and acted with vision and determination is not convincingly demonstrated. Specialists will be disappointed and students will find the book heavy going because the index is poor and so much relevant material is either omitted or relegated to footnotes.

ANTHONY ADAMTHWAITE
Loughborough University

DAVID STRAUSS. *Menace in the West: The Rise of French Anti-Americanism in Modern Times.* (Contributions in American Studies, number 40.) Westport, Conn.: Greenwood Press. 1978. Pp. xii, 317. $22.50.

Anyone who believes that French anti-Americanism disappeared with General de Gaulle need only peruse a random sample of Parisian newspapers to be disabused of that illusion. One will readily discover that the menace of American power continues to haunt defenders of French national independence, from Communists on the left to *chiraquiens* on the right. Anyone thinking that this phenomenon began with de Gaulle is referred to David Strauss's book, which traces the emergence of French anti-Americanism between 1917 and 1933.

Strauss's thesis is that French hostility toward America was a product of the First World War

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and its aftermath. Postwar American policy regarding the Versailles Treaty, disarmament, war debts, reparations, tariffs, loan control, and foreign investments prompted a rapid transformation of the French image of America. Gone was the idealized new world of Franklin and Fenimore Cooper, the frontier settlement of untutored bumpkins looking to the old world for economic assistance and cultural instruction. In its place arose the America of Morgan, Ford, and Hoover, the technological giant striving for world economic domination. Because this policy threatened a France struggling to recover from the ravages of war and to preserve its hegemony in Europe, the erstwhile ally became a treacherous, rapacious predator in the eyes of French observers. Equally as repugant was American cultural penetration of Europe. That Hollywood films and jazz bands could compete with the *Comédie Française* and the *Opéra* for the attention of postwar Parisian audiences added insult to injury.

Strauss reminds us that complaints about the "American menace" during the interwar period emanated largely from the conservative intelligentsia. To these devotees of the old values, the American way of life represented a horrifying model of modernity that Frenchmen might be enticed or compelled to adopt as their own. Taylorism, mass production, the assembly line, and advertising undermined the beliefs of a socioeconomic system based on craftsmanship and qualitative standards. Whereas modern critics such as Servan-Schreiber have regarded the American system as a challenge to be met by adaptation and emulation, earlier observers considered it a menace to be repulsed.

The strength of Strauss's book lies in the restraint and clarity of its presentation, which makes it a readable, entertaining, and informative work. Its principal shortcoming is in its use of sources. It relies almost entirely on the published works of a dozen-odd commentators on American life. Research in the archives, such as the Jusserand and Tardieu papers, would have yielded not only the private recollections of acute observers of America but also an extensive correspondence with various Frenchmen who traversed the Atlantic and reported what they saw and felt. The footnotes reveal a heavy reliance on out-of-date secondary sources, such as Southard (1931) and Williams (1929) on American foreign economic policy, Hayes (1930) on French nationalism, and Moulton and Lewis (1925) and Moulton and Pasvolsky (1932) on war debts. This leaves what I trust is an erroneous impression (partially belied by the bibliography) that the author is unfamiliar with more recent studies of these and related topics.

WILLIAM R. KEYLOR
Boston University

JOSEPH HARRISON. *An Economic History of Modern Spain.* New York: Holmes and Meier. 1978. Pp. x, 187. $22.00.

This book, slight though it is, could not have been written twenty-five years ago. In the past quarter-century the economic historiography of modern Spain has experienced an efflorescence scarcely less remarkable than the renaissance of Spanish economic growth or the still more recent flowering of democratic institutions in that historically backward, benighted nation. The agents of that efflorescence have been a number of mainly younger scholars, both Spanish and foreign. Among the former are several disciples of the late Professor Jaime Vicens Vives (d. 1960), as well as others trained abroad, principally in France and the United States. Joseph Harrison, lecturer in economic history at the University of Manchester, has drawn heavily on the publications of this new generation of scholars, as well as on his own research in primary sources, to produce this synopsis of Spanish economic history since the eighteenth century.

The volume is organized chronologically in eight chapters. Chapter one, "The Economy of the *Ancien Regime*," presents a drastically condensed, relatively static account of the Spanish economy in the eighteenth century; references to the grandeur and decadence of the sixteenth and seventeenth centuries are casual. Chapter two, "The Failure of the Liberal Agrarian Reforms," is not strictly chronological and is less than fully informative about both the nature and consequences of the "agrarian reforms," but it does forcefully make the point that the failure to modernize Spanish agriculture had much to do with Spain's subsequent economic (and political) backwardness. Chapter three, "An Industrial Revolution *Manqué*, 1830–75," concentrates on the establishment of cotton spinning in Catalonia and the building of the first railways, and on the country's inability to generate an ongoing process of industrialization. Chapters four and five, "The Emergence of a Modern Economy, 1875–1914" and "The Formation of a Capitalist Society" (dealing with the period 1914–30), are both mistitled; Spain had neither in 1930, much less in 1914. Chapter six, "The Rise of Organized Labour," like chapter two, is not strictly chronological; it also, by its very existence, exaggerates the importance of its subject. Chapters seven and eight resume the chronological narrative from the establishment of the Second Republic to the death of Franco.

Harrison adheres to a narrative, rather wooden, literary style, inappropriate for macro-economic history and grand generalizations; analytical statements are minimal, and bereft of a knowledge of economics. Most conclusions are presented without adequate evidence. Careless composition results in

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misleading implications; for example, "from 1886 to 1899 636 new companies had been launched . . . each with a capital of 680,000 pesetas" (p. 80), and "the [Franco] regime's ideology proved superior to its achievements" (p. 159). Minor factual errors abound; for example, "Linares Lead, the first foreign company to be founded in Spain, 1862" (p. 55) (not counting the Romans, many foreign companies had been founded prior to that time); and the incorrect date (by thirty years) of the first Watt steam engine to be set up in Spain (p. 59).

Notwithstanding these defects, Harrison's book can be recommended for a brief introduction to the economic history of modern Spain. (I wish I had had it, or something similar, when I began my research thirty years ago.) It complements (and in some respects supersedes) the relevant portions of Vicens Vives's *An Economic History of Spain* (translated into English in 1969) but is less satisfactory if more convenient than Jordi Nadal's and Josep Fontana's contributions to volumes four and six of the *Fontana Economic History of Europe* (1973 and 1976). For readers of Spanish, the excellent bibliography (pp. 175–82) sets forth all the relevant literature.

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W. N. HARGREAVES-MAWDSLEY. *Eighteenth-Century Spain, 1700–1788: A Political, Diplomatic and Institutional History.* Totowa, N.J.: Rowman and Littlefield. 1979. Pp. xi, 181. $18.75.

For English-speaking readers the historiography of eighteenth-century Spain is meager indeed. Although there exists a rich bibliography in Spanish, only a handful of books and articles are available in English. The most glaring deficiency is the lack of a comprehensive account of this era comparable to the works of John Lynch and John Elliott for the sixteenth and seventeenth centuries and Raymond Carr for the nineteenth century. This volume by W. N. Hargreaves-Mawdsley is an attempt to fill that gap, but the author falls short of his objective. He has written a political and diplomatic history of eighteenth-century Spain that excludes any consideration of economic and social developments. Regardless of the author's opinion, stated bluntly in the preface, that "history is men and not metal," any work that claims to be a general account of a period is badly deficient without the inclusion of economic and social history. As a result, this book is a one-dimensional survey of kings, wars, and diplomacy reminiscent of the chroniclers and historians of past centuries. Moreover, it does not contribute any new research findings, nor does it offer any new interpretations.

The book begins with a description of Spanish executive institutions in the eighteenth century to the exclusion of all others. The second chapter deals with the change in dynasty from Habsburg to Bourbon and the War of the Spanish Succession. The emergence of Isabel Farnese as Queen of Spain sets the theme for the period from the Treaty of Utrecht to the death of Philip V. Her ambition to obtain thrones for her sons kept the country involved in Italian wars. The second Bourbon monarch, Ferdinand VI, is viewed as a neurotic, subject to melancholia, who played a small role in government and died mad. The remainder of the book is devoted to the reign of Charles III (1759–88), but the topics discussed are limited to the expulsion of the Jesuits and the wars and diplomacy of the period. The accomplishments of the reign are attributed to the active participation of the king in the government and the support that he gave to such talented ministers as Aranda and Floridablanca.

While this volume is quite dependable as to facts and events, it is laden with personal generalizations and opinionated comments on historical personages. Some of the old clichés and stereotypes are present; for example, "the Spaniard is proud and dislikes change" (p. 2). Individuals fare no better: Cardinal Portocarrero was "a scheming priest" (p. 18), and the Marquis of Louisville, "a foul-mouthed courtier" (p. 21), while the Earl of Peterborough was "brave and impetuous like a Frenchman, but he had plenty of English tenacity" (p. 24). There also was the "sagacious Princess Orsini . . . playing with the astute [Cardinal] Alberoni" (p. 43). Here is a world of court intrigue with individuals pitted against each other and struggling for power and supremacy. Certainly there is more to the history of eighteenth-century Spain, but the author has not been concerned with the broad and deep circumstances of the era. Although written with enthusiasm and clarity, this book is too limited in scope to serve as a textbook and too factual to appeal to a general audience. A comprehensive account of eighteenth-century Spain is still badly needed.

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PAUL PRESTON. *The Coming of the Spanish Civil War: Reform, Reaction and Revolution in the Second Republic, 1931–1936.* New York: Barnes and Noble. 1978. Pp. xiv, 264. $23.50.

The main task that Paul Preston assigned himself in this work was to analyze the fatal interaction between an internally divided Socialist Party and the "accidentalist" right whose principal political leader was José María Gil Robles. According to Preston, all sectors of the Socialist Party in 1931 anticipated a "classic bourgeois" revolution, a transformation offering liberty, economic opportunity,

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and secular social progress to the "petite bourgeoisie," the peasants, and the working class. But no such progressive, democratic capitalism developed after the proclamation of the Republic. By 1933 the Socialists were all disillusioned.

For Julían Besteiro and his followers the Republic had come too soon. The middle classes were not yet ready to perform their historic task, and in any case the Socialist Party should educate and organize for the future, not participate in bourgeois governments. Indalecio Prieto represented the non-Marxists who were primarily concerned with political liberty and economic progress within a capitalist framework. He served in the Republican-Socialist coalition governments of 1931–33, advocated the reconstruction of the coalition from the moment of the rightist electoral victory of 1933, and would have become prime minister in the spring of 1936 if the Caballero wing of the party had permitted him to. Largo Caballero served as minister of labor under Azaña, but after 1933, and especially after the failure of the October 1934 rising (in which he played no part), he adopted revolutionary rhetoric as part of his constant desire to speak for the hopes of both the socialist and anarchist masses.

During these same years, 1931–36, Gil Robles led the one successful mass party of the right. He emphasized that forms of government were less important than the defense of the Church, of property rights, and of strong central (that is, Castilian) government. But his "accidentalism" aroused the scorn of his militant monarchist backers and frightened all committed republicans and the entire left. In detailing the misjudgments, the personal animosities, the use of threatening rhetoric, and the resort to violence, Preston speaks of the Socialists more in sorrow than in anger. For Gil Robles he has nothing but scorn. Very important historiographically is his factual refutation of the widespread idea that Gil Robles (in the 1930s) was solely a parliamentary conservative. Preston shows him considering military "solutions" in August 1932, October 1934, December 1935, and February 1936, but in each case remaining "accidentalist" because the army was not ready to provide the solutions he sought. My single major criticism of Preston's account is that he constantly harps on the protofascist rhetoric of Gil Robles while excusing as infantile and thus somehow less dangerous the "bolshevising" rhetoric of Largo Caballero. But whatever the exaggerations of tone, the main truth stands out. The left preached revolution, and engaged in sporadic violence, but the right planned systematically, from April 1931, to destroy even a mildly reformist, democratic, and capitalist republic.

GABRIEL JACKSON
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JOSEP COLL and JOSEP PANÉ. *Josep Rovira: Una vida al servei de Catalunya i del socialisme.* (Hores de Catalunya.) Barcelona: Editorial Ariel. 1978. Pp. ix, 284.

Josep Rovira was one of the founders and leading activists of the POUM (Workers Party of Marxist Unification), the independent Catalan and Spanish movement of revolutionary socialism that was scapegoated and virtually destroyed by the Soviet Union during the Civil War in Spain. The present volume, written by Josep Coll, Josep Pané, and other POUMist veterans, is a tribute to the memory of their deceased comrade and also a somewhat random collection of mini-memoirs on the POUM. It is not a work of scholarship or an integrated biography of Rovira, much of whose life and career is left in shadows. Altogether, it consists of twenty-five brief chapters written by diverse authors and frequently not well coordinated with each other.

Three primary aspects of the life of Rovira and the party are treated: the founding years before the Civil War, the POUMist militia on the Aragonese front, and finally the activities of Rovira and other POUMists in exile, particularly in the French Resistance. Just as this does not really constitute a full or organized biography of Rovira, neither can it be considered a history of the POUM, for the treatment of both remains fragmentary, scattered, and anecdotal throughout. Readers interested in the record of the POUM itself should consult Víctor Alba's two-volume *Historia del marxismo en España (1919–1939)* (1974) and the subsequently expanded Barcelona edition thereof.

This book will nonetheless be of some utility and interest to students of the Civil War and the Spanish left for bits and pieces of new information that it presents, together with personal reminiscences and an occasional original document. A few new details emerge about the background of the party and the actions of the POUMist militia (with whom George Orwell was briefly associated). The sketch of Rodríguez Sala, the Communist official who as police chief in Barcelona initiated the "counter-revolutionary" May Days of 1937, is perhaps the best that we have to date. Finally, the penultimate chapter presents some documentation on and description of the founding of the "Moviment Socialista de Catalunya," into which the POUM was largely transformed in 1945. In summary, this is not a work of historical scholarship, but it contains material that will be of some interest to scholars.

STANLEY G. PAYNE
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FERNAND SALENTINY. *Aufstieg und Fall des portugiesischen Imperiums.* Vienna: Hermann Böhlaus Nachf. 1977. Pp. 395. DM 76.

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The century heralded in R. H. Major's *The Life of Prince Henry the Navigator* (1868) has witnessed a plethora of monographs on all aspects of that most farflung and enduring of European seaborne empires: the Portuguese. These have ranged from studies of cartographic minutiae to ecopolitics and trade diasporas. Eurocentricity has yielded to analyses of empire building "through the looking glass," of which a distinguished example is David Lach's *Asia in the Making of Europe* (1965–). That the time is ripe for synthesis has been recognized by Vitorino Magalhães Godinho's painstaking scrutiny of Portugal overseas in the fifteenth and sixteenth centuries, Charles Boxer's all-embracing *The Portuguese Seaborne Empire* (1969), and the more geographically, methodologically, and chronologically limited *Foundations of the Portuguese Empire* (1977) of Bailey Diffie and George Winius. That the so-called expansion of Europe has not been so readily treated by German scholars may be partly attributable to the absence of large German overseas possessions. Language barriers have prevented German scholarly works—of which the series *Beiträge zur Kolonial- und Überseegeschichte* (Westdeutsche Verlag, and latterly Atlantis Verlag) affords examples of a high order—from receiving the attention they deserve. Thus it is all the more appropriate to bring to the notice of a wider audience than might otherwise be the case Fernand Salentiny's *Aufstieg und Fall des portugiesischen Imperiums (Rise and Fall of the Portuguese Empire).*

This volume falls into three parts. The opening eighty pages chronicle the background, reconquest, and Portuguese discoveries or claims to primacy from the Atlantic Islands to regions as far removed as Labrador and Australia. The nine chapters of the core discuss, in decreasing order of magnitude (Brazil seventy-four pages; Timor five), the colonial experience in Portuguese America, Mozambique, and Angola. Portuguese Guinea, the Cape Verdes, São Tomé and Príncipe, Timor, Goa, and Macao are dismissed in a mere thirty pages of lip-service. The final ninety-page chapter attempts a balance sheet from Ceuta down to our own day.

All too often Portuguese historiography—plagued on the one hand by the cape-by-cape school and the finding of innumerable havens and on the other by the Hollywood epic school feeding off an inexhaustible diet of shipwreck and siege—has not only distorted reality but distracted from what was by any yardstick a fantastic achievement. International pressures and domestic considerations that honed colonial policy are too easily forgotten, as too is the delicate interplay between the component parts of empire. Salentiny's publications, and they are legion, range from an encyclopedia of sailors and discoverers; to the lives and deeds of adventurers, explorers, freebooters, and merchants since 1000 A.D.; to a volume on the rise and fall of Carthage. They suggest the tone of the volume under discussion: long on facts, short on analysis, and with a low level of generalization. The section on the discoveries recounts an oft-told story, taking the reader once again through the by now burnt-out controversies of primacy, motivation, and secret voyages. The Cook's tour of the colonies is pedestrian and the parallel treatment of political, economic, and demographic development is not likely to stir interest. Far from providing a balance sheet, the final chapter—predictably divided into rise, consolidation, and fall—affords few insights. The role of missionaries as carriers of culture is discussed over-lengthily, the American dimension is dismissed with undue haste, and ventures beyond the seventeenth century are undertaken only reluctantly. The reader in search of the preconditions and precipitants of the decline of empire will be disappointed. The promise of the preface that the author will provide historical dimensions to enhance our understanding of recent events in Africa and the vertiginous haste of what was somewhat fancifully referred to as "exemplary decolonisation" lies unfulfilled.

This work is not representative of contemporary scholarship; in style, methodology, and emphasis it is reminiscent of a bygone age. If one takes the bibliography at face value, it is based on extensive reading of secondary sources, yet not only is the bibliography somewhat outdated, but also there are unforgivable omissions. The persevering reader will turn to Diffie and Winius for a more complete and up-to-date rendering. Salentiny deluges the reader with a flood of facts of varying degrees of accuracy (the participation of Tiradentes in the War of the Peddlers might raise some eyebrows [p. 111]), but this volume is plagued by inumerable errors in Portuguese names and words. All too accurately the indexes are designated as registers of places and people. The single greatest shortcoming is the absence of a map. With due caution, Salentiny may be used as a work of reference or to provide quick background, but, despite their less broad canvases, the earlier studies of Boxer and Diffie and Winius will prove more rewarding to the reader who wishes to know more of the blood chemistry of the most enduring of European empires.

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HERMAN VAN DER WEE and EDDY VAN CAUWENBERGHE, editors. *Productivity of Land and Agricultural Innovation in the Low Countries (1250–1800).* Leuven: University Press. 1978. Pp. viii, 187. 580F.

This collection of nine specialized statistical essays on agricultural productivity in the Low Countries, with an introductory synthesis by Herman Van der

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Wee, consists of papers presented to the Seventh International Congress of Economic History. The authors use land books, population figures, tax records, and particularly tithe statistics, these being of considerable value as indexes of relative productivity despite the fact that tithes were generally farmed. The authors are wisely wary of taking their statistics at face value, particularly those derived from tithe figures, and they apply the appropriate internal and external corrections in computer-assisted analysis. The project was inspired by Emmanuel Le Roy Ladurie and uses the methods suggested by his general study (1972) of the tithe.

The authors are perhaps too cautious in extrapolating from the particular locality studied to general conclusions for larger regions. Yet the nine studies offer conclusions that are surprisingly consistent in view of the wide variety of areas covered, from the polders of North Holland to the labor-intensive grain areas of southeastern Flanders and the Meuse basin near Liège. Grain yields remained roughly stable during the late medieval depression; in contrast to other regions, there was neither serious decline after 1300 nor rapid recovery in the fifteenth century. Productivity per hectare expanded in the early sixteenth century as population grew and agriculture became more intensive to meet rising demand. Since seed yields were considerably higher in the Low Countries than in most other parts of preindustrial Europe, the extreme fractionalizing of plots there created a less serious problem than might be supposed.

Seed yields peaked in the older settled areas of grain monoculture in the early sixteenth century and did not rise again appreciably until the massive use of chemical fertilizers in the late eighteenth and nineteenth centuries. The authors reject the classic view of Slicher Van Bath that a rapid rise in grain yields occurred at the end of the Middle Ages. On other crops, however, yields rose until the religious warfare from 1567 led to a catastrophic drop. The early seventeenth century witnessed some recovery, particularly in the northern Netherlands, but depression in the urban economy with a diminished market for farm produce meant a general decline in the agrarian sector until the mid-eighteenth century in most regions. The authors find a general correlation throughout the period among levels of farm productivity, rents and prices of land, farm prices, and the overall profitability of agriculture for landowners. Despite short-term fluctuations, the authors find a general absence of economic growth in the Low Countries throughout this period, although the fact that most of their statistics are for grain production may weaken this conclusion to some degree.

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E. H. KOSSMANN. *The Low Countries, 1780–1940.* (Oxford History of Modern Europe.) Oxford: Clarendon Press. 1978. Pp. ix, 784. $39.00.

For most historians most of the time the small countries remain *terra incognita.* That they weigh relatively little in the scale of grand politics is beyond doubt; but it is also true that within their borders major themes of historical development are worked out with unusual clarity, perhaps because in the eternal interplay of foreign and domestic politics the former plays generally a lesser role than in major countries and thus interferes less with internal affairs. That their history merits the attention of historians outside their borders is demonstrated in this history of the two countries that comprise the Low Countries—the Netherlands and Belgium—by a Dutch historian of unusual gifts of mind and pen.

Ernst H. Kossmann, who taught for some years at the University of London before returning to the Netherlands to take up a chair at Groningen, did not turn to this task out of the impulsion of his own intellectual needs but at the request of the editors of the Oxford History of Modern Europe, of which this book forms the seventh volume published. The assignment confronted him with what has become in this century the central question in the history of both countries, the nature of their relationship all the way back to the early Middle Ages. In the fifteenth and sixteenth centuries, when the Low Lands by the Sea began to emerge as a politically and culturally distinctive region, the neohumanist name of Belgium was given to it. The unity it achieved under the Burgundian dukes was sundered by the revolt that began in the 1560s; restored in 1815, the union lasted only a decade and a half and persisted as a historic memory that seemed far less important than the alienation between the Dutch and the Belgians (a name henceforth given only to the inhabitants of the southern Low Countries). The recency of their separation was part of the essential historical message of Pieter Geyl, but he excluded the French-speaking Walloons from his vision of the Great Netherlands. Only after the shared experience of invasion and occupation in World War II did the countries (together with Luxemburg) come together in the unity of Benelux and did historians on both sides of the border treat their history as in essential respects one.

Yet it is a unity that cannot deny deep and apparently permanent division. Kossmann does not inflict an artificial singleness upon his subject; he accepts as given the difference of character that separate life has given the two intimately related lands. He finds their unity not so much in the memories of the Burgundian state as in the common language of a majority of their population and in their experience as small states thrust into the very center of the

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European great-power struggle. He constructs his account to fit this general pattern. He does not attempt to force events into a single narrative, except for the brief period of unity under King William I, but presents the two stories in close parallel, shifting from one to the other within significant episodes of a few decades each. He compares these developments constantly, achieving an integration of vision unprecedented in the historical writing of both countries. His account of the role of neutrality in the politics of both countries will not be unfamiliar to most historians in its broad outline, although they will learn much from its detailed course. What will be new for most is the "school struggle," as it was called, the contest, that is, between state and private (religious) schools that dominated political life for most of the nineteenth and early twentieth centuries. Equally new, probably, will be the phenomenon of *verzuiling* ("pillarization") in Dutch history, the splitting of almost all cultural and social as well as political life along lines of religious-ideological identification, making the Netherlands the exemplar of pluralist society in the Western world. (The present crisis of "pillarization" is well beyond the terminus of Kossmann's book, but the subtlety of his analysis of its rise and triumph makes one eager for his judgment of what may be its last phase.)

The mood of his vision is irenic and contemplative. He has no particular argument to make, no side to uphold or condemn. Yet he obviously feels deeply about the intensely human content of the events he studies; it is the breadth of his empathy rather than any lack of involvement that enables Kossmann to present so many different participants, singular and collective, with such remarkable fairness. A work like this must inevitably rest chiefly upon secondary sources, but Kossmann handles them with superb skill. He draws from them not only factual data chosen for their direct significance and implications but also interpretations that contribute to his overall picture. It is evident that he is Dutch and not Belgian, not because of any national partisanship but because he is more at home in the history of his own country than in that of Belgium; but this has led him if anything to study the Belgian story even more closely.

The work was written originally mainly in Dutch and then translated by the author himself into English; the Dutch version (*De Lage Landen, 1780–1940* [1976]) preceded the appearance of the English text. If Kossmann's English style lacks the pungently idiomatic quality of his Dutch prose, it remains vigorous and clear. I found myself prompted to check the original Dutch perhaps a dozen times and found only a very few cases where the author as his own translator was thinking in Dutch though writing English. The English text is somewhat longer, primarily to provide information that a Dutch or Belgian reader would not need. But to all it will provide a fullness of information and a freshness of understanding that can come only from a master of the art of history. It is good that it is available to more than his countrymen and their Flemish companions.

HERBERT H. ROWEN
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JOHAN DE VRIES. *The Netherlands Economy in the Twentieth Century: An Examination of the Most Characteristic Features in the Period 1900–1970.* Translated by DEREK S. JORDAN. (Aspects of Economic History: The Low Countries, number 3.) Assen: Van Gorcum. 1978. Pp. ix, 135.

Why is twentieth-century economic history so difficult to write well? An old cliché blames the lack of "perspective" that turns recent history into a branch of journalism. It is more probable that the difficulties are caused by the nature of economic events in the twentieth century. It is the history of great disequilibria caused largely by human folly and fanaticism. Economic analysis is ill at ease with the consequences of the actions of a Stalin, a Mussolini, a Bethmann-Hollweg—or, for that matter, a Herbert Hoover or a Hendrik Colijn (the central Dutch political figure during the Great Depression). As a result, economic analysis is often jettisoned altogether from twentieth-century history, at great cost in terms of coherence and comprehensibility.

Johan De Vries, the author of a seminal work on eighteenth-century Dutch economic history, has written a little book that scratches the surface of Dutch economic history in the twentieth century. The original Dutch version purported to reach scholars and students alike. The English version loses its appeal to scholars because the footnotes are omitted, and students will be turned off by the clumsy and incompetent translation. Many an elegant turn of phrase in the original Dutch is mutilated into awkward and at times simply erroneous or unintelligible English.

The first part of the book, on "structural changes," is standard and a bit dull. The second part, which deals with business cycles, is more interesting, especially the surprisingly rich treatment of the economic effects of the two world wars. The most interesting part of the book is the third one, which deals with economic policy. The story is one that will sound quaintly familiar to American readers. A liberal economy, clinging desperately to the axiomatic principles of free markets and laissez faire, was caught in the maelstrom of depression and war. The result: a "governmental habit," more easily acquired than shed. Considering the comparative efficiency and integrity that characterizes

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the present Dutch welfare state, one wonders why economic regulatory agencies in the U.S., which emerged under seemingly similar circumstances, have become clumsy and corrupt giants. Regrettably, this question has not occurred to De Vries.

It is unclear why no attempt is made to place the development of the Dutch economy in an international context. The discussion of the EEC is sketchy and references to Dutch economic relations with its neighbors or its colonies are few and brief. No mention of the impact of guest laborers, multinational corporations, or capital exports is made. Moreover, the writings of non-Dutch economic historians are ignored. There is no evidence of any input of the works of such pioneers as Ragnar Nurske, C. P. Kindleberger, W. A. Lewis, and Ingvar Svennilson. The bibliography does not contain a single item dealing with other countries.

Good twentieth-century economic history is hard to write. It is to be hoped that this introductory booklet on an interesting economy will stimulate more work among those courageous economic historians who are willing to take up the challenge of recent economic history seriously.

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INGRID HAMMERSTRÖM *et al. Ideologi och Socialpolitik i 1800-talets Sverige: Fyra studier* [Ideology and Social Policy in Nineteenth-Century Sweden: Four Studies]. Summaries in English. (Acta Universitatis Upsaliensis. Studia Historica Upsaliensia, number 103.) Uppsala: Historiska Institutionen vid Uppsala Universitet; distributed by Almqvist and Wiksell. 1978. Pp. xv, 475. 79 KR.

These four essays are concerned with the relationship between social issues and the political ideology of Swedish legislators, officials, and interested citizens during the mid-nineteenth century.

"Controlling the Lower Classes: The Question of 'Defenselessness,' 1825–53," by Väinö Helgesson, traces the efforts of the authorities to deal with the growing number of Swedes without property or unemployed. Such persons could be forced to accept contract labor for begging, "continued laziness and dissolution," or wandering "about in an unauthorized manner" (pp. 438–39). Helgesson shows that, although some employers saw these vagrants as a source of cheap labor and therefore supported strict legislation and enforcement, it is nearly impossible to draw a clear distinction between liberals and conservatives as they debated and voted on this problem. "Indeed . . . it was the conservatives who most vigorously defended the rights of the nonpropertied" at the Parliament of 1840–41 and in the newspaper debate (pp. 437–38). The fact of the matter was that the laws simply did not work, and there was constant conflict over who should pay the cost—the local authorities or the national government.

"The Debate on Child Labor in Industry and the Crafts, 1850–83" by Barbro Hedvall concludes that it "is simply not possible" to classify "the advocates and opponents of protective legislation either according to the arguments they used, their social or economic position, or their political sympathies" (p. 443). Some manufacturers in Gothenburg took the initiative for protective legislation in 1852. When the government issued a decree on child labor in 1881, it came under severe attack. The upshot was that the sawmill and mining interests forced the Parliament to exclude these industries from the decree.

Christer Knuthammar's "Health Care Under Changing Circumstances: Finance and Administration of Swedish Hospitals, 1850–73," reveals that there was virtually no ideological debate. Hospitals and venereal disease sanitoriums were for the poor and they carried the financial burden. The "changing circumstances" did not change the health care during the years studied in this essay but rather made possible a vastly expanded hospital system later on when revolutions in medicine required hospitals of a modern character. One example of changing circumstances was the local government reform of 1862 giving the county the power of taxation.

"The Elementary School System of Sweden, 1842–72" shows that the church and the conservatives wanted the schools to remain, by and large, "a forum for Christian training" preparing the children for holy communion, "book reading, writing, and simple arithmetic" (p. 447). Although liberals argued for a school system promoting individual choice, their desire for local control came into conflict with the need for state money to foster a good system in the poor districts. Eventually they sacrificed local control to the state, albeit not entirely. The national government paid the salaries, while the commune was responsible for all other expenses as well as administration of the state guidelines. Of the four issues discussed here, this was the only one with a clear ideological dichotomy.

Ingrid Hammarström, project director for these studies, has done an excellent job. Her introduction to the summaries in English (pp. 433–49) clearly delineates the scope and methodology followed. Three of the four studies examine in more detail the conditions vis-à-vis the laws in a particular county. A unified bibliography and a name index are included. Readers of English at last have something to compare to the reform debates in Great Britain during the mid-nineteenth century.

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KLAUS WITTMANN. *Schwedens Wirtschaftsbeziehungen zum Dritten Reich, 1933–1945.* (Studien zur Modernen Geschichte, number 23.) Munich: R. Oldenbourg Verlag. 1978. Pp. 479.

Regarding the publications on German international economic policy during World War II—and the years before—the economic relations with Sweden up to now have not elicited as much interest as have those with other countries. Only the Swedish export of iron ore was discussed incidentally in some papers. The book in hand is the first attempt by a German author to write a comprehensive study of economic relations between Sweden and the Third Reich from 1933 to 1945. Based on numerous materials from German archives, the book gives a detailed picture of all those problems that were involved in German-Swedish cooperation during this period.

Klaus Wittmann starts with a historical description of Schacht's "New Plan" in 1934 and the beginning of bilateralism. After this time it was not iron ore alone but also wood, agricultural products, ball bearings, machinery, and—with the beginning of war—transport capacity that the Germans wanted, while Sweden needed German coal and rolling-mill products. But the exchange of goods, even under the influence of the four-year plan, was restricted by currency control, clearing arrangements, and Germany's attempt to free itself from old foreign loans, such as the "Kreuger loan" that Sweden had granted to the Weimar Republic. With the beginning of the war, problems of armaments and supply came well to the fore for the Germans, while Sweden tried to preserve its neutrality in trade in spite of massive pressures exerted on the Swedish government by Germany and the Allied countries. The Germans succeeded in concluding a war trade agreement (December 22, 1939), and, after the occupation of Denmark and Norway, the encirclement of Sweden and its isolation from world trade favored the German idea of a *Grossraumwirtschaft*, a new order of European economy under German direction. Wittmann discusses Sweden's position in the different plans for this new organization of the European economy in great detail (pp. 204–40). In 1942, when the military and political difficulties of Germany matured, Sweden tried to make closer contact with the Allies; the skill shown in these efforts prevented the very real possibility of a German occupation of Sweden.

All this is described by the author, who makes clear to us that Sweden had to be open to trade with Germany as well as to trade with other parts of the world. Not only Germany—regarding the imports of iron ore, ball bearings, and the use of transport facilities for the troops in Norway and Finland—but also Sweden was interested in an uninterrupted exchange of goods. Arrangements with Germany and the Allies had to secure a minimum supply of goods for the Swedish economy. Wittmann describes in detail the so-called Göteborg traffic, for example, which allowed Sweden to send out ships in spite of German and Allied blockade. Although Sweden was willing (or forced) to make concessions toward Germany during the first part of the war, Allied pressure increased after the U.S. entered the war. In spite of further deliveries to Germany, corresponding to the last Swedish-German arrangement of December 1943, Sweden succeeded in reducing trade with Germany until all exports were stopped on December 31, 1944.

The book gives a very good description of the way in which Sweden had to find a path between the opposing powers in the war, always aware of the danger of involvement in that war. Furthermore, Wittmann uses the Swedish example to demonstrate the principles of German foreign policy as a great power in Europe.

HARALD WINKEL
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RAIMO RANTA. *Pohjanmaan maaseudun käsityöläiset vuosina 1721–1809.* Volume 1, *Käsityöläiseksi pääsy ja käsityöläisten lukumäärä* [Artisans in the District of East Bothnia in the Years 1721–1809. Volume 1, Entry into the Artisans Occupation and the Number of Artisans]. Summary in German. (Historiallisia Tutkimuksia, number 108.) Helsinki: Suomen Historiallinen Seura. 1978. Pp. 312.

At first glance this academic thesis (Turku University, 1978) seems a formidable challenge: two hundred eighty-six pages of text; eight pages of bibliography; four hundred twenty-five footnotes (two hundred and one in a single chapter); sixty statistical tables with eleven more in the appendix; fifty graphs; virtually a parish-by-parish analysis with the names of hundreds of artisans and others (genealogists take note!).

But after reading Raimo Ranta's book, one concludes that it is an important and fascinating study of rural handicraftsmen in the twenty-eight parishes of Pohjanmaa province in 1721–1809. The author's major sources are tax rolls, various church documents, records of assize courts, parish meetings, and so forth. These clearly are not easy to work with; there are numerous gaps and inconsistencies. Almost continuous warfare resulted in changing situations; laws and regulations were often ignored or indifferently implemented.

There were in all nearly three thousand rural handicraftsmen in Pohjanmaa during the period, of whom 1,552 had official permits and paid occupa-

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tional taxes, and 1,427 practiced a trade without authorization. Their numbers fluctuated widely. The three major categories (and for a time the only legally permitted rural callings) were tailors, blacksmiths, and shoemakers. By the turn of the nineteenth century the number of trades had risen to a score or more. The five largest groups, other than the three mentioned above, were carpenters (74 in 1721–1809), master-builders (67), masons (42), turners (28), and furriers (23). Peasants often practiced the more common callings without approval or paying taxes.

Official permits came nominally from the governors upon the recommendation of assize courts and parish meetings. Need was the foremost criterion; skill followed, although proof was not always required. Craftsmen often came from their own parishes: sons of crofters, tenants, and small landowners. Some had learned their trades from fathers; others served a three-to-five year apprenticeship. Many became craftsmen to escape the greatly feared military conscription.

Of interest to emigration scholars is Ranta's careful analysis of demographic changes (Pohjanmaa's population swelled from 72,113 in 1749 to 195,978 in 1805 despite famines and wars), which were to provide the surplus rural elements from which the bulk of the nineteenth-century emigrants to America were to be recruited.

A forthcoming volume will deal, *inter alia*, with the economic status of handicraftsmen, their social and geographical mobility, shifts to other callings, and relations between rural and urban artisans.

This highly recommended investigation merits more than a mere four-page German language summary.

JOHN I. KOLEHMAINEN
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WERNER BERTHOLD *et al.*, editors. *Kritik der bürgerlichen Geschichtsschreibung: Handbuch.* 4th rev. ed. Cologne: Pahl-Rugenstein Verlag. 1977. Pp. x, 751. DM 48.

To write a satisfactory history of modern historiography is very difficult, because the pertinent literature has become so vast. The last single scholar to succeed in this task was George P. Gooch, who, at the age of ninety, presented a revised German version of his magnum opus, *Geschichte und Geschichtsschreiber im 19. Jahrhundert* (1964). The *Handbuch*, however, was compiled by fifty-seven East German historians, competent scholars; but different from Gooch, who aimed at impartiality, they never left any doubt about their inspiration by Marxism-Leninism. The first edition of the *Handbuch* (1970) was very aggressive in its criticism of West German scholarship. The present version, which includes literature up to 1976, is less vindictive in tone without changing its basic Communist tendency. Yet hostile phrases such as "Rankes Pseudoobjektivität" (p. 89), "verfälschte Darstellung der Oktoberrevolution" (p. 592) and "antisowjetische, nationalistische Geschichtsklitterung" (p. 685) are found on almost every page and spoil the enjoyment of the publication.

The *Handbuch* claims to be the standard work of the historical sciences of the German Democratic Republic and deserves to be respected as such. The first edition was sold out within a year and the next two were also in heavy demand even in West Germany. The present fourth edition shows great improvements. It is better organized and since it is almost two hundred pages longer, it has space for important additional subjects. Among them are, for example, the French Revolution, the German revolution of 1848, imperialism before 1914, cultural and economic historiography, the Communist International, and the history of the Russian Communist Party and its East German affiliate, the SED. Recent trends in West German historical writing are also more carefully analyzed.

Even a very stout volume cannot do justice to such a great variety of topics as the *Handbuch* addresses. The reader will be disappointed to find the *Ranke-Renaissance* covered in one short paragraph and only eight of its leaders mentioned by name, without discussion of their individual merits. Something should have been said here about Lenz and Oncken. Or what do we gain by seeing sixteen names that are to represent the "deutschnational-völkischen Flügel der imperialistischen Interessenpolitik" (p. 91), among them, rather oddly, Herzfeld and Rothfels? Nor was Hintze an "alldeutschen Praktiken anhängender Historiker" or Holborn "rechtsliberal" (p. 92). More justice is done to the leading historians of the postwar generation, Conze, Erdmann, Golo Mann, and Schieder and the men who are now gaining prominence, such as Bracher, Hans Mommsen, Wehler, and Nipperdey. Also, English and American scholars like Toynbee and Barraclough, Carlton Hayes and Hans Kohn are not overlooked. The inclusion of many Russian historians will be helpful. Interesting is the chapter on *Weltgeschichte*. An appendix contains useful information about West German historical organizations, institutions, and journals as well as pertinent East German and Russian publications.

With all its obvious shortcomings, this is an important work for scholars concerned with modern German historiography. It would be desirable to have a West German compilation of similar scope, but with much less bias, if any.

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HAROLD J. GRIMM. *Lazarus Spengler: A Lay Leader of the Reformation.* Columbus: Ohio State University Press. 1978. Pp. xii, 237. $15.00.

To his many writings on the German Reformation, Harold J. Grimm has now added a meticulous account of the life and work of Lazarus Spengler of Nuremberg, council secretary in the first third of the sixteenth century, staunch Lutheran, resourceful politician, and a strong activist in the cause of reforming his home town. As secretary, Spengler participated in the governing council's privy sessions, kept its records and conducted its correspondence, gave legal and financial advice, and took diplomatic assignments. Few men in Germany could have had a better grasp of urban politics and administration. An adherent of Luther almost from the beginning, he distinguished himself as an advocate of "evangelical" reform by publishing in 1519 one of the earliest popular explanations of Lutheran doctrines and by urging the council to make appropriate religious changes, notably the appointment of Lutheran preachers. From then on he devoted his talents and influence to steering the Reformation on a course he considered safe and godly: breaking away from the old order without giving in to demands for radical change made by an aroused populace and by disaffected intellectuals and maintaining the city's independence of action without alienating the emperor or falling out with other Protestant Estates. This was a difficult balancing act and not a popular policy in the inflamed atmosphere of the 1520s and 1530s. In arguing for it, Spengler rested his case on Scripture and his conviction that the empire must be maintained intact. The Bible, he insisted (against the interpretation of others), forbade the use of force in the pursuit of political aims; armed rebellion against the authority of the emperor was therefore out of the question. As for the empire, Spengler was certain that the survival of (increasingly anachronistic) independent city states depended on the endurance of the imperial system, which, he thought, was being fatally undermined by Protestant leagues. His memoranda, reports, and dispatches therefore counseled a policy of conservatism at home and nonalliance abroad. It was a posture that his city's patrician governors found congenial, although in the end it contributed significantly to the decline of Nuremberg's influence and importance in the second half of the sixteenth century.

Spengler's life passed without any of the grave crises or stirring climaxes that make for exciting biography. He emerges from this book as an ordinary man who worked hard and fought honestly for what he believed, and Grimm's low-keyed narrative underlines this unpretentious style of conduct. Against methodological challenges from many fronts, Grimm has remained an unreconstructed practitioner of *histoire événementielle* who does not encumber his story with much analysis or comment. He moves his hero efficiently through the stages of his career, sketching in the necessary background so that students in a Reformation course will be able to handle the book easily. On most matters Grimm adopts Spengler's point of view; author and subject are never far apart in their objectives.

While I learned much about Spengler from this study (much more than from Hans von Schubert's 1934 monograph), I have some doubt as to whether urban history in sixteenth-century Germany is best studied by means of this traditional approach. Without intending to do so, Grimm's book reveals the collectivist nature of German city politics in the early modern period. If Spengler cuts a pallid figure, no one else among his city's burgher-aristocrats, lawyers, pastors, and humanists (with the possible exception of Willibald Pirckheimer) stands out in sharper relief. The German city did not tend to produce conspicuous personalities. It neither promoted nor tolerated magnetic leaders and eminent shapers and shakers. Grimm calls Spengler "a lay leader of the Reformation," but his book does not convey an impression of compelling leadership in the conduct of affairs. Spengler did not stamp his personality on his work, nor did he intend to, nor did any of his prominent fellow citizens. Leadership in Nuremberg was homogeneous, the product of class, family tradition, and the urge to guard privilege and position. It distrusted individualism and sought to subdue it even among the notable. Biography is not the best key to how such a system works.

GERALD STRAUSS
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KLAUS MÜLLER. *Das kaiserliche Gesandtschaftswesen im Jahrhundert nach dem Westfälischen Frieden (1648–1740).* (Bonner Historische Forschungen, number 42.) Bonn: Ludwig Röhrscheid Verlag. 1976. Pp. 407.

Klaus Müller carries on the work of diplomatic historians such as Garrett Mattingly, David Bayne Horn, and Max Braubach, his mentor, in *Das kaiserliche Gesandschaftswesen im Jahrhundert nach dem Westfälischen Frieden (1648–1740).* He maintains that the century after 1648 marked a breakthrough in international diplomatic practice. Although there were resident missions throughout Europe prior to 1648, it was only after the Treaty of Westphalia that the major European powers established an extensive network of permanent embassies. Müller undertakes to explain the functioning of the imperial diplomatic system within this larger European context

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by discussing such universal problems as inadequate pay, divided loyalties, and espionage. He succeeds in depicting the actual mechanics of imperial diplomacy by analyzing such basics as diplomatic correspondence, court institutions, and promotion and ranking within the diplomatic service. A subtle weighing of ancestry, religion, wealth, and ability determined entrance into and advancement within the diplomatic corps. The most important positions, he stresses, were allotted to Catholic nobles from the Habsburg *Erblande*.

Müller has done for the imperial diplomatic service what Horn has done for the British. The constitutional position of the imperial diplomats, the impact of the French and Swedish systems, the personal involvement of the emperor (especially Charles VI), and the priority accorded to Austrian as opposed to imperial policies are but a few of the interesting highlights of this work. The evidence he cites confirms what many historians have long suspected. This book is based on painstaking research, conducted chiefly in the Haus-, Hof-, und Staatsarchiv in Vienna. An impressive bibliography, helpful index, and extensive footnotes aid the reader. Without doubt this monograph will prove indispensable to diplomatic historians.

MARSHA FREY
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JOHN T. O'CONNOR. *Negotiator out of Season: The Career of Wilhelm Egon von Fürstenberg, 1629 to 1704*. Athens: University of Georgia Press. 1978. Pp. x, 263. $20.00.

This study offers a re-examination of one of the more controversial minor figures in seventeenth-century Germany. Wilhelm Egon, the most durable and talented of the three Fürstenberg brothers, played a significant role in the secular and ecclesiastical politics of the Holy Roman Empire, pursuing his own interests as he did those of his patron, Louis XIV. He served as bishop of Strasbourg, long-term member of the cathedral chapter, and unsuccessful candidate for Archbishop Elector at Cologne.

John T. O'Connor traces in intricate detail Fürstenberg's role in complicated diplomatic and political maneuvers, basing his research upon materials in the French archives, the Vatican, Vienna, and a few smaller collections, as well as the large published literature on the period. This study is a solid piece of work, the first full-length treatment of Fürstenberg in English, and one that will be useful to students of the life style of the aristocratic prince-bishops of the empire as well as to historians of diplomacy. The conclusions are not startling, but the evidence is weighed carefully, and a clearer picture of French diplomatic activity in Germany emerges as Wilhelm's activities are delineated.

O'Connor thoughtfully analyzes the contradictions inherent in Fürstenberg's self-chosen role as a German prince and agent of the French king, a division of loyalties that even contemporaries found sometimes disturbing. The author concludes that despite Wilhelm's assertion of a German princely liberty independent of the Habsburgs, Fürstenberg "stretched to the limits the elasticity reserved for princes in the constitution of the Empire. He may have been less than patriotic, but to brand him a traitor pure and simple is to ignore too many obstacles."

Since Fürstenberg left no personal papers, and the author had to work with the more circumspect official correspondence, it is difficult for him to penetrate to his subject's deepest personal motives. O'Connor makes a serious effort to do so, and he does make the German response to Louis XIV's diplomacy and intrigue more comprehensible. The study is also helpful in clarifying Pope Innocent XI's intentions in promoting the succession in Cologne of Joseph Clemens of Bavaria rather than Fürstenberg.

O'Connor's work does not supplant, though it usefully supplements, Max Braubach's biography of Fürstenberg. It is a valuable addition to the literature on seventeenth-century Germany and should be of interest to anyone doing work on the internal or diplomatic history of the period.

ROGER WINES
Fordham University

JOHANNES KUNISCH. *Das Mirakel des Hauses Brandenburg: Studien zum Verhältnis von Kabinettspolitik und Kriegführung im Zeitalter des Siebenjährigen Krieges*. Munich: R. Oldenbourg Verlag. 1978. Pp. 154. DM 38.

Was Prussia saved from almost certain dismemberment by the fortuitous death of the Empress Elizabeth of Russia in 1762? Johannes Kunisch attempts to answer this question and to determine why a formidable coalition of powers was unable to defeat Frederick the Great and his army by 1762. Maria Theresa hoped, initially, to reduce Prussia to its earlier, second-rate status. Intimidated by the militarism and vicious diplomacy of Frederick, she wished for revenge but did not advocate dismemberment of the Prussian state because such an action would gravely disturb the balance of power in Europe and would lead to further, dangerous rivalries of great powers. The Tsarina Elizabeth, reacting similarly, disliked the Prussian power position in the Baltic and wished to enlarge Russian influence in that region. In short, in 1756, considerations of balance of power forced the allies to preserve a state that constituted a great danger to their interests.

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By 1761 both Maria Theresa and Elizabeth believed that Prussia might, indeed, be forced to disgorge not only Silesia but also East Prussia, Prussian Pomerania, Magdeburg, and the Rhineland enclaves. But Prussian defense of Silesia in that year, combined with a timidity of generals, did not enable the allies to construct an effective campaign. By the end of the year diminishing material and human resources made allied statesmen and generals as reluctant to enter chancy battles as their Prussian opponents. Maria Theresa revealed pessimism concerning the outcome of the war as early as 1759, when she observed with dismay the defeat of the French overseas and the failures of the allies in the Continental campaigns of 1757 and 1758. By 1761 she merely hoped that Frederick would be forced to return Silesia. Whereas Austrian generals were overly impressed by contemporary theories of maneuver and overfearful of battle, Frederick was not bound by rules or convention. Ruling Prussia and its armies through interior lines of defense, Frederick was able to act more quickly than his enemies, who were divided in leadership and by geography. By 1762 mutual exhaustion characterized all protagonists and the death of Elizabeth was only the occasion for all parties to agree to disengage from combat.

Although Kunisch's work contains little that is novel in detail, it does develop an interesting and convincing thesis. Maria Theresa and her policies are particularly well discussed, but the policies of Elizabeth and of Louis XV are left undeveloped. The discussion of military theory is quite orthodox and ignores examples of total warfare as waged not only by the Prussians but also by the allies as well during the war.

HUBERT C. JOHNSON
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HANNA SCHISSLER. *Preussische Agrargesellschaft im Wandel: Wirtschaftliche, gesellschaftliche und politische Transformationsprozesse von 1763 bis 1847.* (Kritische Studien zur Geschichtswissenschaft, number 33.) Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 285. DM 48.

Historians' assessments of the Prussian reform era of 1807–15 almost invariably conclude that the reform movement was a failure. Thanks to several recent studies, one of the most convincing of which is Hanna Schissler's *Preussische Agrargesellschaft im Wandel*, such interpretations should soon become obsolete. It is not that the reforms were successful, but rather that "success" and "failure" are inappropriate terms of evaluation for this period of intensive modernization in Prussia. As Schissler correctly points out, historians who have employed such language have themselves usually identified with the ideals of the reform movement and have thus lacked an appropriate perspective for assessing its impact on society.

In contrast, Schissler, like many contemporary West German historians, employs conceptualization and methodology developed in the social sciences. She evaluates the applicability of several models and theories such as "modernization," "protoindustrialization," and "economic take-off" to Prussia during the transition she studies. Her work examines the transformation of agricultural society over an eighty-year period, the midpoint of which is the reform year, 1807–08.

Schissler's interpretation can be summarized in the following way: by the end of the eighteenth century, Prussian agriculture had undergone a process of "commercialization," which meant highly intensified farming for export markets. Noble estate owners had carried out this innovation at the expense of peasants and urban dwellers. The consequent agricultural boom produced an economic crisis that threatened the social structure of Hohenzollern lands. Simultaneously Prussia suffered defeat and severe fiscal difficulties in the Napoleonic war of 1806–07. The reform bureaucracy led by Stein and Hardenberg sought to save the state, and, in doing so, accelerated the modernization process already underway by permitting the moneyed middle class to enter the field of agriculture. While this represented a threat to the social privilege of the aristocracy, it strengthened their economic position by encouraging the growth of large estate farming.

The reforms promoted a transition from commercial to capitalist agriculture. This caused dislocation of large segments of the rural lower classes, producing a rigid social division between propertied peasants and the landless agrarian population, and in some cases it caused severe poverty. In the short run the transformation also restrained the growth of industry in Prussia rather than fostering it, as many other "agricultural revolutions" have done.

Schissler's excellent study, synthesized from published sources and secondary literature, contributes to a long overdue reinterpretation of the Prussian reform era. More significantly, it is an important case study that will interest scholars of many specializations who investigate the transition of traditional agrarian societies to modern industrial ones in both Western and Third World settings.

MARION W. GRAY
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CARSTEN BRODERSEN. *Rechnungsprüfung für das Parlament in der konstitutionellen Monarchie: Ein Beitrag zu den Auseinandersetzungen um die Ausgestaltung des parlamentarischen Budgetrechts in Preussen-Deutschland, 1848–1877.*

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(Schriften zur Verfassungsgeschichte, number 25.) Berlin: Duncker and Humblot. 1977. Pp. 243. DM 69.

The control of state income and spending was a central question of German constitutionalism in the nineteenth century. In Prussia the constitution of 1850 decreed that the diet had to pass a budget annually and required an audit as an additional check on the executive. The constitution further promised a statute creating an auditing office. In the interim, the General Audit Office (*Oberrechnungskammer*), an institution established under absolutism and directly responsible to the king, continued to check government accounts according to its old guidelines.

Carsten Brodersen's detailed work, based on published sources, traces the attempts to establish an audit system that would have given the diet more insight and, thereby, control over government spending. The diet and the General Audit Office collaborated in the management of the state debt. Otherwise, the office produced summary audits and strictly refused to give supplementary information on the grounds that it was accountable only to the king. The promised statute, enacted in 1872, did little more than formalize the established auditing procedures. A minority of principled deputies failed to make the General Audit Office more answerable to the diet. The Prussian auditing system, apparently little more than an exercise in balancing the books that was of no political consequence, was also adopted for the imperial finances.

Brodersen notes that already in the 1870s constitutional experts played down or ignored the earlier notion that the audit was an important adjunct to the budgetary powers of the diet. This position is understandable. A government under Bismarck, which had shown so little respect for the budget, would have respected an audit even less.

The absence of a conclusion to this meticulously crafted study points to its major weakness: like the Prussian auditing procedures, the study gives no significant new insights into the struggle for power at that time between the monarch, his cabinet, and the diet. This problem might have been partially remedied if Brodersen had been able to use the records of the Prussian cabinet and diet, which are now in East Germany. The problem, however, lies also in the topic of his investigation. The outcome suggests that the legal-constitutional approach to Prussian-German history is becoming subject to the law of diminishing returns.

ERICH HAHN
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KARL OTMAR FREIHERR VON ARETIN, editor. *Bismarcks Aussenpolitik und der Berliner Kongress.* (Institut für Europäische Geschichte Mainz.) Wiesbaden: Franz Steiner Verlag. 1978. Pp. 162. DM 8.

Otto von Bismarck was notorious for his malignant opinion of mankind, and among the victims of his condescension were the Balkan peoples, who lived in a backwater that Lord Palmerston, with equal contempt, had once dismissed as "a barbarous province at the end of Europe." The only significance that this benighted area possessed for Bismarck was as the place where a conflagration between the rival ambitions of Russia and Austria-Hungary might occur should one or the other attempt to assert a dominating position there. Bismarck consequently was thoroughly alarmed by the rapacity of Russia's demands for Ottoman territory following the defeat of Turkey in the war of 1877–78. The Treaty of San Stefano, which had enshrined Russia's ambitions, had to be set aside in favor of a more measured settlement. With the cooperation of France and Great Britain, Bismarck convened the Congress of Berlin in June 1878. After a month's deliberations the participants formulated a treaty that deprived Russia of many of its spoils of war. The resentment felt in St. Petersburg, pointedly expressed by Tsar Alexander II, Gorchakov, and the organs of the pan-Slav press, centered on the German chancellor. The result of Bismarck's concern over what he regarded as Russia's irresponsible appetite in the Balkans and of his irritation over the Russian attacks made on him and Germany was his decision, made late in 1878, to seek a defensive alliance with Austria-Hungary, which envisioned the Tsar as the putative aggressor.

Bismarck's behavior in 1878–79, including not only his approach to Vienna but also the anti-Socialist law, tariff revision, the break with the National Liberals, and the attenuation of the Kulturkampf, has for many years been the subject of a historical controversy, which is traced in an attractive contribution by Lothar Gall on "Bismarck in der Geschichtsschreibung nach 1945." Was the chancellor at this juncture in his career merely the representative of economic and social forces, or was his personality primarily responsible for imparting to imperial Germany in 1878–79 the form and character it would thereafter possess? If the latter, should Bismarck be regarded as an essentially positive factor, satisfying but also controlling in his time the engine of radical German nationalism? This was the evaluation of Hans Rothfels, and it was challenged by Erich Eyck and others, who argued that the chancellor was in fact the nemesis of German liberalism. Gall rightly cautions historians against allowing Bismarck to disappear in the shade of the social and economic forces that Helmut Böhme, Hans Rosenberg, and Hans-Ulrich Wehler have forcefully analyzed. While Böhme and his fol-

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lowers acknowledge the chancellor's skill as a politician and diplomat, Bismarck at their hands is forced to operate within the constrained context of these superior, impersonal agencies. Bismarck, for all his magisterial command of his contemporaries, emerges as a subaltern actor in the historical process.

The role of Bismarck at the Congress of Berlin is an episode that does not provide much shelter for those who would treat him as a historical factor of secondary importance, and there is little in his activity there to substantiate the claim made here (and elsewhere) by Imanuel Geiss, in an otherwise lucid essay, that his diplomacy was fashioned by the exigencies of internal problems. Bismarck's behavior at the congress was shaped by his own diplomatic preconceptions of Germany's relationship with the other powers, not by economic or social interests, and further refined by his contacts with the foreign plenipotentiaries who were his partners in negotiation. Who can say what form the final settlement might have taken with regard to Russia had the fussily prideful Gorchakov, whom Bismarck loathed, possessed even a scintilla of Disraeli's captivating charm? As Theodor Schieder points out in an enlightening discussion of Bismarck's assumptions on the European balance of power in 1878 and afterwards, the chancellor's diplomacy was a reflection of his determination to provide his new empire with security and to preserve for himself the adjudicating role that he had played at the congress. In this way, any attempts by France to form a coalition directed against Germany could be quashed swiftly, tensions that threatened to lead to war could be transferred to the periphery of Europe or even to overseas areas, and Russia could eventually be wooed back into cooperation with Germany and the Habsburg Empire. The success of this strategy was revealed in the 1880s, when France was detained in diplomatic isolation, imperialism distracted the attention of the powers from the Balkans, and Russia returned to the web of alliances that were Bismarck's greatest achievement.

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L. L. FARRAR, JR. *Divide and Conquer: German Efforts to Conclude a Separate Peace, 1914–1918.* (East European Monographs, number 45.) Boulder: East European Quarterly; distributed by Columbia University Press, New York. 1978. Pp. x, 180. $12.00.

Since the late 1950s, hundreds of monographs in at least half a dozen languages have been devoted to Imperial Germany's policies, attitudes, and experiences during the First World War. One subject, Germany's "war aims policy," has been investigated with particular thoroughness and passion, with the focus above all on two questions: (1) what exactly did the Kaiser's government and various segments of German society hope to gain from the war Epolitically, economically, and/or territorially); and (2) how did these aspirations shape or affect the general conduct of the war by the civilian and military leaders of the Reich.

As might be expected, the most detailed studies in this field have been written by German scholars. Wolfgang Steglich's *Bündnissicherung oder Verständigungsfrieden* (1958) and his subsequent *Die Friedenspolitik der Mittelmächte 1917–1918* (1964) together cover well over 800 pages. Almost as long are Karl-Heinz Janssen's two contributions, *Macht und Verblendung* (1963) and *Der Kanzler und der General* (1967). Fritz Fischer's famous *Griff nach der Weltmacht* was nearly 900 pages long when it first appeared in 1961 (it has since been revised, abridged, and translated into English). Finally, there are Gerhard Ritter's findings, almost 1,300 pages of text and notes, in the last two volumes of his *Staatskunst und Kriegshandwerk* (1964–68). This work, too, has since been translated into English.

In *Divide and Conquer*, L. L. Farrar, Jr., once more goes over the story of when, how, and why the German government tried to lure or pressure Russia and some other Allied powers into a separate peace. He concludes that "all German leaders sought to divide the Entente so as to conquer the continent and establish German world power" (p. 106); that "no moral distinction should be drawn between German soldiers and statesmen" (p. 113) just because the latter were more moderate in their demands; and that Germany's wartime diplomacy was based more often than not on faulty assumptions about the attitudes and aspirations of the Allied leaders and their nations.

Farrar's account of what Germany's leading figures thought, planned, and did to divide the Entente is reasonably complete, and some of his comments along the way are very good indeed. There are a few factual errors in his story, and the backnotes are rather uneven in quality. One would also have wished for the inclusion of an index. For the general reader, however, this concise overview of certain key aspects of Germany's wartime diplomacy will undoubtedly be of both interest and value.

ULRICH TRUMPENER
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DAVID R. LIPTON. *Ernst Cassirer: The Dilemma of a Liberal Intellectual in Germany, 1914–1933.* Buffalo: University of Toronto Press. 1978. Pp. xi, 212. $15.00.

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David R. Lipton's study may not satisfy philosophers looking for a detailed exposition of Cassirer's philosophy, but it will be of great value to historians interested in the fate of German liberalism in the twentieth century. As an intellectual biography of the last Marburg neo-Kantian, it also contributes to the growing literature on that important movement.

The method of the book is hermeneutical, in the informal sense, because the author explores the meaning of Cassirer's neo-Kantian (and increasingly Hegelian) philosophy of culture in the evolving context of German politics. Using neglected texts and unpublished materials as well as Cassirer's major works, Lipton manages, as he claims, to tie Cassirer's ideas "directly to historical events." Cassirer, for example, felt compelled to stress the cosmopolitan dimensions of German classical culture when he was writing *Freiheit und Form* during World War I. He was consciously trying to counteract the chauvinism expressed in the "ideas of 1914" and to remind his compatriots of their European cultural heritage. The last book he wrote in Germany, his study of the Enlightenment, was a similar response to the imminent destruction of reason and denial of the humanist past.

During the Weimar period Cassirer's thought was a barometer of middle-class liberalism. According to Lipton, Cassirer achieved a balance in his philosophy between abstraction and fact, between universal and particular, only during the years of recovery and stability. As the republic began to totter and the electoral fortunes of the German Democratic Party sank, this equilibrium was lost, and Cassirer suffered an intellectual "relapse" into the pre-1914 dogmatic rationalism of the Marburg school. He retreated into the logical formalism that had kept the neo-Kantian movement in rarified isolation from the mainstream of German political life. After Cassirer moved beyond the narrow range of neo-Kantianism to take an interest in the "whole man" by making an "Hegelian correction" of Kant, the impending collapse of liberalism made him take refuge in pure concepts as found in the third volume of his *Philosophy of Symbolic Forms*. The preceding volumes were full of factual content; the third was disappointingly abstract.

There is an explicit critique of intellectual liberalism and neo-Kantianism running through the book, and it amounts to an indictment of those closely related philosophies for their elitism, intellectualism, and impracticality. The neo-Kantians and, in particular, the legal philosophers inspired by them, such as Stammler and Kelsen, sought pure norms of law. They were unwilling and unable to establish practical norms for judging positive law. Indeed, the entire neo-Kantian movement was characterized by what Lipton calls the formalist fallacy, the habit of dissolving reality into abstract categories, splitting ethics from politics, and ducking the responsibility to offer practical solutions to real problems.

Intellectual historians will quarrel with his dismissal of Marburg neo-Kantianism as hopelessly aprioristic. Cassirer's mentors, Hermann Cohen and Paul Natorp, were, in fact, greatly concerned about practical matters. Cohen, doyen of the Marburg group, called for democracy in Prussia in place of the plutocratic three-class voting system, and he was also interested in tax reform. Natorp wrote extensively on the theory of education and elaborated a theory of social pedagogy. There was a serious, if ultimately unproductive, alliance between Marburg neo-Kantianism and socialist revisionism before 1914. Karl Vorländer, another Marburg disciple, tried to popularize Kant's personalist ethics in order to build a bridge between the social liberalism of Marburg and the socialist movement.

Neo-Kantianism was not as far from the real world of politics as Lipton maintains, and his section on the neo-Kantian background is the least satisfactory part of the book. He mentions Otto Liebmann and F. A. Lange, without discussing Lange's political activities in the 1860s. It was really Lange who founded the Marburg school and gave it a genuine if unfulfilled pragmatic orientation. Timothy Keck's Wisconsin dissertation on Marburg socialism, which does not appear in Lipton's bibliography, would have enriched his understanding of Cassirer's background. These reservations notwithstanding, Lipton has written a lucid account of the relationship between Cassirer's thought and the German political environment.

THOMAS E. WILLEY
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SUSANNE MILLER. *Die Bürde der Macht: Die deutsche Sozialdemokratie, 1918–1920.* (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 63.) Düsseldorf: Droste Verlag. 1978. Pp. 532.

After her study of German Social Democracy during World War I (*Burgfrieden und Klassenkampf* [1974]), we now have Susanne Miller's account of how a divided German working class after the war briefly exercised political power for the first time in its history. "The Burden of Power" is a fitting, because revealing, title for a story that begins with the entry of majority socialists into the last imperial cabinet of Max von Baden in October 1918; it continues with an account of how the SPD and USPD tried to cope with the subsequent collapse of the empire, the revolution, and the sudden and essentially unwanted responsibility of government; and it

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ends by showing how, after two years of trial, error, and good intentions, Social Democracy returned in June 1920 to its traditional, familiar, and therefore "safe" prewar stance: political opposition.

The story, as the author ably shows, is tragic. Germany's Socialists inherited the power of government unexpectedly as a result of a revolution not of their own making. When the decisive moment arrived they were split into two parties; worse, they were unprepared for the tasks ahead. Neither the SPD nor USPD had planned politically for what to do if power should fall to them, nor did they possess enough qualified persons to fill governmental posts. As a result, the two years in power—in which the USPD after December 1918 no longer shared—were marked by *Zerrissenheit*, halfway measures, and dubious policies. Divisiveness was everywhere: between SPD and USPD, provisional government and council movement, Reich government and federal states, among others. Although committed to a socialist program that predated the war, Social Democracy in power failed to carry out most of its provisions, including socialization of industries. To govern meant for the SPD in particular to "react to national catastrophes" (p. 445); it never welcomed power as the fulfillment of a long-desired objective.

Unpreparedness to deal effectively with foreign and domestic crises runs through the account like a red thread, and while the author argues convincingly that extenuating circumstances were responsible for failure to handle the former, she comes down hard on the SPD's unimpressive record with regard to the latter. The party was unprepared for, and unable to deal with, the increasing radicalism of the masses in the spring and summer of 1919, the result of discrepancies between expectations and actual achievements of the revolution. These discrepancies the party failed to eliminate, though it had created them by its prewar propaganda and its postwar policies. A socialist government that evaded socialization and did not impose democratization on bureaucracy, judiciary, or the army (whose repressive measures against radical workers, sanctioned by the SPD, created particular resentment) was bound to lose credibility. Throughout 1919 and early 1920 discontented workers flocked into the USPD, which, although the "party of protest" throughout, proved unable to exploit this human windfall because it suffered from internal division and indecisive leadership.

Throughout her book the author has struck an admirable balance, both between narrative and interpretation and in her presentation of achievements and shortcomings of Germany's first socialist experiment in government. She has visited fifteen archives in six countries, and her bibliography of published sources as well as her statistical appendixes are exemplary. While the book offers no startlingly new discoveries, it is more than a synthesis of existing studies in that the author's archival research adds many new insights to specific aspects of the topic. The one weakness, the sketchy presentation of relations between SPD and its Weimar Coalition partners during 1919–20, is a minor flaw in view of her overall achievement.

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HERMANN GLASER. *The Cultural Roots of National Socialism.* Translated by ERNEST A. MENZE. Austin: University of Texas Press. 1978. Pp. 289. $12.95.

Hermann Glaser's well-known polemic, *Spiesser-Ideologie*, originally published in 1964, is here translated and edited for an American public. In this, as in a great many other writings over the last fifteen years, Glaser laments the decline of the German intellectual traditions of the early nineteenth century. By 1850 or so, he believes, the ideas of the Classical and Romantic periods had come to be misunderstood or emptied of meaning, resulting in philistine mediocrity throughout German intellectual life.

This perversion of ideas into pseudo-ideas is, according to Glaser, central to an understanding of National Socialism, a thesis he defends by juxtaposing, more or less at random, quotations from men as diverse in time, status, and talent as Wagner, Rilke, Treitschke, Arndt, Wilhelm II, Ernst Jünger, and, of course, Adolf Hitler. Such juxtapositions, the author claims, demonstrate the "structural" similarities between the thought of Hitler and that of various nineteenth- and early twentieth-century writers on such subjects as the nation, Jews, sex, family, youth, blood, leadership, home town, and so on. The author rarely pauses to analyze his quoted materials or to discuss their provenance, nor does he confront questions of historical development or change. The book is, then, a compendium of selected clichés of literature and public oratory and depends for effect not on argument but on reiteration. It is not history, nor is it intended to be: Glaser describes it as an attempt to redefine what is acceptable in the German intellectual tradition, so that contemporaries may reclaim it "without qualms of conscience." But, since Glaser excels in poking fun at clichés, not much emerges as acceptable beyond what is vaguely described as "German warmth, German spirit and German culture" (p. 13).

Glaser, an official in the education department of the City of Nuremberg, is a pithy and popular writer. His works, which include attacks on the provincialism of the right (*Kleinstadt-Ideologie* [1969], *Der Gartenzwerg in der Boutique* [1973]) and on radical

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youth groups of the left (H. Glaser ed., *Jugend-Stil, Stil der Jugend* [1971]), give considerable insight into the intellectual and ideological quandaries that have faced Germans of the moderate left in the last two decades. Ernest A. Menze's introduction discusses the book in this light, but the usefulness of this edition would have been greatly increased had he commented at greater length on the controversies Glaser's work has aroused—on its context and contemporary significance. Glaser's volume is of interest to students of current patterns in German thought; it is not, despite the unfortunate English title, either a serious or significant study of the origins of National Socialism. Apart from the title, the translation is adequate, but it loses both the shrill tone and the pungent vigor of the original.

BARBARA MILLER LANE
Bryn Mawr College

ULRICH DUNKER. *Der Reichsbund jüdischer Frontsoldaten, 1919–1938: Geschichte eines jüdischen Abwehrvereins.* Düsseldorf: Droste Verlag. 1977. Pp. 354. DM 48.

Based on a Free University of Berlin doctoral thesis, Ulrich Dunker's book expands the literature of twentieth-century Jewish resistance to anti-Semitism. The *Reichsbund jüdischer Frontsoldaten* (RjF), a 35,000-member "defense league," was the second-largest Jewish organization in interwar Germany. Less influential than either the larger *Centralverein deutscher Staatsbürger jüdischer Glaubens* (CV) or the more activist *Zionistiche Vereinigung*, the RjF's history, according to the author, offers new insight into the failure of the "German-Jewish symbiosis" (pp. 9, 185). Because the RjF's archives were destroyed by the Nazis, Dunker has searched *Reich*, *Land*, and municipal records in East and West Germany; testimonies of former members in Israel, North and South America; RjF publications; and extensive secondary literature.

Though rooted in the Second Reich, the RjF was an outgrowth of the new, more virulent anti-Semitism of World War I: the Jews, made the scapegoat for prolonged war and the blockade, for defeat and revolution, were denounced as profiteers, traitors, and, especially, for shirking military service. The RjF was founded on February 8, 1919, to defend the honor of Jewish front soldiers, their right, and that of all German Jews, to be accepted as full-fledged citizens. It began with fifty-one veterans who had been rejected by *völkisch* servicemen's organizations and discouraged by the CV. Stressing its German nationalism, the RjF fought the Spartacists, patrolled Hamburg during Christmas 1920, and accompanied the Free Corps in Upper Silesia. Led by Reserve Captain Dr. Leo Loewenstein, by 1925 it grew to five hundred local and sixteen regional organizations, with a national assembly and twelve-member governing board, consisting of attorneys, professors, a journalist, and one estate owner. Its newspaper, *Der Schild*, preached comradeship, courage, and austerity.

Dunker divides RjF defense tactics into the "deed" and the "word." During the November 1923 attacks on Berlin Jews (triggered by agitators as well as inflation and unemployment), the RjF armed, protected synagogues, and fought the assailants. It promoted self-defense through sports, boxing, and gymnastic societies, sponsored agricultural settlements, and led patriotic demonstrations. When anti-Semitic incidents mounted through the "prosperity years" 1925–28, it joined the secret, paramilitary *Jüdischer Abwehr Dienst* (JAD).

Its propaganda (*Aufklärungsarbeit*) aimed at disarming anti-Semitism with facts, cited the war record to prove that Jews were neither cowardly nor "unproductive." The RjF published books, trained speakers, and printed posters with *völkisch* language and iconography, including an "Appeal to German Mothers." Its most ambitious venture was a 1932 list of ten thousand Jewish war dead. An "unpolitical" organization that shunned Jewish affairs (especially Zionism) and confined itself to opposing anti-Semitism, it developed no strategy to halt Nazism's spread or to defend the Weimar Republic.

After Hitler seized power, the RjF grew, reorganized into an authoritarian structure, and publicly supported the regime. Its early, spectacular triumph—Jewish veterans were exempted from removal from the Civil Service—was nullified by the Nuremberg laws. Its *raison d'être* evaporated when all Jews were excluded from the *Wehrmacht*. Transformed from ardent nationalists to persecuted Jews, RjF refugees were aided by Zionists and by English-Jewish servicemen's groups. Banned by the Gestapo, the RjF ended at *Kristallnacht*, when resisters disappeared into concentration camps.

Dunker balances criticisms of the RjF's elitist and defensive tactics under Weimar and its doomed opportunism under Hitler with the assertion that its basic aims were shared by many German Jews, even after 1933. Yet, the average RjF member remains a stereotype: had he other loyalties that occasionally conflicted with the organization's ideal? How did he compare with his French, English, or American counterpart? Dunker concludes that the RjF's ultimate assimilationist goal might have succeeded in "more tranquil times" except for the "unique historical process" that brought Hitler to power (p. 185), but his evidence suggests a more pessimistic conclusion.

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PETER D. STACHURA, editor. *The Shaping of the Nazi State.* New York: Barnes and Noble. 1978. Pp. 304. $18.50.

Peter D. Stachura's *Shaping of the Nazi State* constitutes a welcome addition to the growing literature about the National Socialist movement and the nature of Hitler's Third Reich. It is welcome in the sense that it makes available to us nine original and avowedly revisionistic essays on this subject, all of which are based on hitherto unexplored archival material, while simultaneously affording a number of younger historians an opportunity to present their work in print.

Chapter one, by Geoffrey Stoakes, examines Hitler's ideas on foreign policy prior to his publication of the first volume of *Mein Kampf.* Chapter two, by Albrecht Tyrell, discusses the enigmatic career of Gottfried Feder and his failure to become an important ideologist and leader of the Nazi Party. In chapter three, the editor, Peter D. Stachura, undertakes an interesting analysis of "Der Fall Strasser," concerning Gregor Strasser's development as an independent political figure within the Nazi movement and his abortive 1932 attempt to bring the party into a coalition of parties headed by General von Schleicher. Conan J. Fischer, in chapter four, presents a most interesting study of the occupational background of the S.A. membership for the years 1929 to 1934 and suggests that, unlike the party (which was dominated by the petty bourgeoisie), the S.A. was predominantly working-class in derivation. The rise of the National Socialist Students' Organization and its failure in the realm of political indoctrination in the universities of Nazi Germany is the subject of Geoffrey J. Giles. This is paralleled by Jill Stephenson's contribution in chapter six regarding the organizational success of the Nazi Women's Organization that was astonishingly devoid of ideological impact on most German women. Jeremy Noakes's contribution deals with the 1936 Crucifix Struggle of Catholics in Oldenburg and represents a most interesting case study of the parameters of success and failure of the resistance against the National Socialist regime. In chapter eight Jane Caplan subjects the "dualism" between the civil service bureaucracy and the Nazi Party to a closely reasoned examination and poses the question whether the governmental chaos in Hitler's Germany was not as much the fault of the bureaucracy as the party's constant intervention. Finally, in chapter nine, Marcus S. Phillips explores the financial and ideological role of the German film industry in planning and executing Hitler's "New Order" for Europe.

As is generally the case in compilations of this sort, individual contributions vary considerably in quality and interest. All of the essays, however, ask important questions and reach interesting and new conclusions. This is especially true of Jeremy Noakes's essay on the Catholic school struggle, which represents a veritable model for the application of local history to the analysis of broader questions concerning the possibilities and limitations of the resistance against the Nazi regime.

This collection of essays could have profited from a more comprehensive introduction. More rigorous editorial control over the unfortunately low literary quality of some of the essays would have considerably enhanced its readability. Despite these shortcomings, however, the *Shaping of the Nazi State* will add a provocative new dimension to the existing literature.

DANIEL HORN
Rutgers University, New Brunswick

MATTHEW COOPER. *The German Army, 1933–1945: Its Political and Military Failure.* New York: Stein and Day. 1978. Pp. x, 598, $17.95.

In 1943, when the fortunes of war were still in doubt, Major General J. F. C. Fuller described Adolf Hitler as "one of the most, if not actually the most, original soldiers in all history." The unsoundness of this judgment has been pointed out by many writers, including such associates of Hitler as General Alfred Jodl and Field Marshal von Manstein; but Matthew Cooper's new book is surely the most extended repudiation of the Fuller view. Cooper insists that Hitler's utterances reveal no real insight into the nature of war, that he possessed no coherent, detailed strategy, and that, far from possessing a revolutionary doctrine, he had only the vaguest sort of military philosophy, his approach being very much that of "the military illiterate and political street fighter, the former with his fascination for numbers, destructive power, and speed of operations, the latter with his belief in the importance of deception, brute force, and propaganda" (p. 126). Against weak and confused opponents, these attributes were effective. But once the war had settled down to a long slogging match against a determined alliance, they were powerless, and Hitler's basic weaknesses as *Feldherr* appeared in their most damaging form—lack of appreciation of the limitations imposed by space and time, underestimation of the capabilities of his antagonists, indecisiveness in operations, lack of strategic vision, distrust of subordinates, and a belief that power of the will could overcome all problems.

The greater part of Cooper's book is given over to a circumstantial elaboration of these charges. Some readers may feel that he verges on the overzealous as he goes about this, particularly when he claims

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that *Blitzkrieg* was a myth, that neither Hitler nor senior commanders of the army understood the proper uses of armor, and that Hitler deserves no credit for preventing a retreat after his armies ground to a halt before Moscow in the winter of 1941. But his arguments are shrewd and his descriptions of operations, particularly on the eastern front, beautifully clear and vigorous. His analysis of the steady crumbling of the strength of an army that was never properly equipped for the efforts demanded of it is highly effective, as is his discussion of the shattering psychological effects of such unwelcome surprises as the appearance of the KVI and T34 tanks in the Soviet line of battle in 1942, machines that were vastly superior to the best German ones.

The second theme of Cooper's book is the systematic degradation of Germany's military leaders in the Hitler period, a process that began with their oath of allegiance to the Führer in August 1934 and ended with their transformation into mere executants of his orders, without initiative or even the power to counsel and advise. "No body of generals," General Geyr von Schweppenburg once wrote, "was ever so thoroughly divided, watered down, disunited, and internally disintegrated." Why did they tolerate this, particularly after they had come to realize the certain results of Hitler's amateurish conduct of the war? Cooper inclines to the view that they were paralyzed by their oath, but more perhaps by an essential weakness of character and a nagging self-doubt that had been induced by early experiences in which Hitler had been right and they had been wrong. For whatever reason, they allowed their consciences to become dulled and obeyed orders that they knew were unsound. For this obedience they earned nothing but Hitler's contempt, and, Cooper writes, they deserve the same from history. Theirs was "the greatest folly in the history of the German Army."

GORDON A. CRAIG
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CHARLES W. SYDNOR, JR. *Soldiers of Destruction: The SS Death's Head Division, 1933–1945.* Princeton: Princeton University Press. 1977. Pp. xvi, 371. $9.95.

Charles W Sydnor, Jr., has written a splendid study of the evolution and wartime activities of one of the most notorious of Nazi Waffen-SS divisions, that which eventually became the Third SS Panzer Division *Totenkopf.* It is an impressive example of scrupulously documented administrative and battlefield history. Sydnor leads the reader through the intricacies of the division's formation in the months between the conquest of Poland and the campaign in France in the spring of 1940, where *Totenkopf* made its combat debut. The division's role in subsequent operations—almost exclusively on the eastern front, where it, along with the whole of the Waffen-SS, came of age—is described in narrative chapters that reflect a mastery of the relevant primary and secondary sources. The most that can be said in criticism of this effort is that Sydnor occasionally may tell some readers more than they want or need to know.

But this book is more than a divisional history. Woven throughout the narrative is a convincing demonstration of *Totenkopf*'s close association with the SS-run mass-murder system. To the reader who is not familiar with the complexities of Heinrich Himmler's "SS state," this might appear an exercise in belaboring the obvious. Were not all SS men members of the same nefarious brotherhood, and do not all of us know what *its* purposes were? But the degree to which the Waffen-SS, which fought alongside the German Army, was an organic rather than merely nominal component of the *Gesamt-SS* has long been a focus of some disagreement, and Sydnor contributes important insights and information to the debate. *Totenkopf*'s roots reached deep into the Nazi underworld of ideologically charged mayhem and death. Its first commander, Theodor Eicke, had occupied a central position in the development of the concentration camp network, first as commandant of the Dachau camp and, ultimately, as inspector of the entire camp system. His fierce hatred of all of Nazism's adversaries and his enthusiasm for pathologically brutal methods of dealing with those adversaries were internalized by his subordinates and carried into the combat division when it was organized with camp personnel as cadre. The result, Sydnor argues, was a grisly catalogue of atrocities. Throughout the war, moreover, the division's links to the world of SS criminality behind the lines were frequently reinforced by the movement of personnel between concentration camps and *Einsatzgruppen* and the front.

Apologists for the Waffen-SS (most of them veterans of that organization) have long maintained that SS front-line fighters were "soldiers like any others" (to quote the title of one particular apologetic), by which they mean in part that these troops were no more guilty of atrocious conduct than other World War II combat formations. For them, this book may make unpleasant reading. Yet, perhaps it is time for a serious comparative study of World War II criminality from an international perspective. Such an undertaking would not exculpate Nazi Germany and would sharpen our understanding of the difficult problem of battlefield morality.

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JOSEPH ROVAN. *Histoire de la social-démocratie Allemande.* (L'Univers Historique.) Paris: Éditions du Seuil. 1978. Pp. 524.

It has become almost a rule that Social Democrats receive bad marks from historians, both at home and abroad. The way Joseph Rovan, professor at the Paris university of Vincennes, treats their party forms a striking exception. His avowed purpose in writing this history is to prove that democracy and civil liberties can flourish under social democratic governments (p. 10).

The book is remarkable both for its scope of reference and for the stance it takes. It embraces a period of well over a century, covering the development of a party that, at its early stage, was a sociopolitical as well as a cultural movement with revolutionary aims; it became the only stable pillar of the shaky Weimar Republic and has been wielding governmental power, together with the Free Democrats, in Bonn for the last decade.

More than half the book is devoted to the time after World War II. The previous chapters dealing with the rise of German Social Democracy from Lassalle's small Workers' Association to the largest member party of the Socialist International, its internal conflicts over the interpretation of Marxism, its split due to its policy in World War I, its failure to save the first German republic, and its opposition to the Hitler regime are largely based on secondary literature. While the presentation in those chapters is accurate and intelligent but somewhat pedestrian, the second part is fascinating to read, for the author is a keen observer, passionately interested in the contemporary scene. His portraits of Kurt Schumacher, Willy Brandt, and Helmut Schmidt stand out for their analytical quality. Rovan's appreciation of the 1959 Godesberg program of the SPD and of the political achievements of the governments led by SPD Chancellors Brandt and Schmidt culminates in his appeal to the Social Democrats to have the courage to follow that course unswervingly and to assert that Ebert was right against Liebknecht, Blum against the protagonists of the Third International, Schumacher against Pieck and Ulbricht, Ramadier against Thorez (p. 480). In short, this book spreads the message that Social Democrats must at all times withstand any temptation to give in to Marxists and Communists.

Of course, this message is addressed in the first place to Social Democrats in France, since for most German Social Democrats it is merely stating the obvious. The question is whether Rovan's approach to his subject will serve his intentions. This is highly doubtful. The book has a fundamental weakness that results from the author's zeal to prove that in the long run the Social Democrats' political decisions were never wrong, or, if they were, such as Schumacher's foreign policy, they were put right in the course of time. By this Rovan avoids a critical assessment of the highly controversial issues in the history of the party, such as the majority socialists' accommodation to government policy in World War I, their rigidity toward dissenters in their own ranks that led to the split of the German labor movement, Noske's blind trust in the reactionary military, and the SPD's lack of energy when faced with the rising Nazi tide. In dealing with the present-day SPD, Rovan sees its further sucesses menaced solely by the left-wingers inside the party, while he tends to excuse political mistakes for which the leadership was responsible. The case for the SPD in past and present could have been put across more convincingly if the author had refrained from apologies on matters that deserve a more critical examination.

SUSANNE MILLER
Bonn, West Germany

DAVID P. CONRADT. *The German Polity.* (Comparative Studies of Political Life.) New York: Longman. 1978. Pp. xviii, 235. Cloth $12.50, paper $6.95.

Capturing the contemporary reality of an entire political system in a book of modest size is a challenge quite different from writing a specialized monograph. Whichever road an author chooses, he or she is bound to leave out themes other scholars treasure. David P. Conradt's *German Polity* succeeds admirably in drawing together a well-balanced view of the present post-World War II West German system, although it would be easy to find topics the author had to leave aside or to find fault with a title that blissfully ignores the other German state. The book owes its impression of cogent unity to two features. One is the consistent effort to relate the discussion of each major social group to how much it has come to identify with the democratic republic by today. The other feature is the heavy reliance on the author's specialty, the scientific and quantitative study of public opinion and the attitudes of groups. This is Conradt's forte, and he handles it and other statistical aspects of his subject with authority and style. To the extent that a living political system can be defined as a system of interrelated group attitudes, this book is truly excellent and exhaustive.

The German Polity also shows unexpected strength on such less accessible subjects as foreign workers, interest groups and parties, *Bundestag*, bureaucracy, planning, and local government in the federal republic. It is less profound on the institutions of German federalism and on German legal traditions, specialities that used to give unity and cogency to earlier accounts, such as the writings on Germany of Carl J. Friedrich or Karl Loewenstein. Inevitably,

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some important subjects receive short shrift in *The German Polity*, as in the dated description of the Young Socialists (pp. 73–75, 87) or when the author comments on the excess of international class identification among West Germans as compared to their identification with the nation (p. 48). Finally, there are some awkward translations such as "case worker" (*Sachbearbeiter*), "current hour" (*Aktuelle Stunde*), or "clogging" (*Verfilzung*) that make the reader wish the author had been content with simple descriptions. But all these flaws are minor in character and should not detract from the author's major achievement, namely, to have supplied a sound, new, social-scientific picture of the West German polity.

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CHARLES W. INGRAO. *In Quest and Crisis: Emperor Joseph I and the Habsburg Monarchy*. West Lafayette, Ind.: Purdue University Press. 1979. Pp. xi, 278. $12.95.

This study of the reign of Joseph I (1705–11) offers a welcome addition to the literature on a period long dominated by historians writing from either a French or a British viewpoint. Charles W. Ingrao has distilled a mass of documentation from Austrian, German, Hungarian, and British archives to produce a well-constructed, compact survey of Viennese court politics and Habsburg strategy.

Joseph's sudden death from smallpox in 1711 posed enormous problems for European statesmen trying to sort out the Spanish succession. It also left many problems for historians by interrupting a promising reign in mid-career. *Grossdeutsch* historians have seen in Joseph an ardent reformer, protonationalist, and precursor of Frederick the Great. Ingrao's work shows convincingly how these views must be qualified. Although Joseph was unlike his father, Leopold I, and his brother, Charles VI, in temperament, taste, and conventional piety, he still responded to his office much as they did, promising what he must when he had to, delaying fulfillment of promises when it served his interests. He perceived the threat to imperial power presented by the powerful electors in Germany, encouraging the smaller states to counterbalance their influence. Lacking his father's sense of dynastic mission, Joseph showed little enthusiasm for helping his brother Charles win the throne of Spain; yet his Italian policy displayed all the universalist ambitions characteristic of Habsburg family tradition. While he was willing to grant religious liberty in Hungary to end a rebellion, he showed elsewhere, particularly in Silesia, his reluctance to abandon the advantages presumed to derive from a dominant Roman church.

The main political developments are carefully and, on the whole, clearly sketched. Joseph's personality and the degree to which he initiated imperial policy are harder to judge. The assessment of Joseph's own role as "an arbiter rather than as the dynamic force in the formulation of state policy" (p. 225) is probably correct and says a good deal about the character of Habsburg government at the beginning of the eighteenth century. However much Joseph appeared to differ from his father, there is probably more continuity of policy than Ingrao admits. While Joseph may well have had more political intelligence than his father or his successor, and certainly a more vigorous and positive view of his own function as monarch, his mercurial temperament and unpredictable behavior often nullified his influence, leaving the direction of affairs in his ministers' hands. Taken together with Max Braubach's and Derek MacKay's works on Prince Eugene, this book helps clarify a number of important questions about the development of Habsburg absolutism, without making the common mistake of assuming that it was Eugene, not Joseph, who ruled the emerging Danubian empire.

JOHN P. SPIELMAN
Haverford College

EVA HOLLEIS. *Die Sozialpolitische Partei: Sozialliberale Bestregungen in Wien um 1900*. (Österreich Archiv.) Munich: R. Oldenbourg Verlag. 1978. Pp. 122. DM 19.80.

By the 1890s bourgeois liberalism in Austria was in disarray; its once-central place in political life was increasingly overtaken by anti-liberal movements: Catholic-conservative Christian Socialism, racist German nationalism, and Marxist Social Democracy. In her brief essay, Eva Holleis relates the attemps in Vienna to establish a new style of social liberalism in which democratic principles and social reform would occupy prominent positions. Formed in the mid-1890s to contest communal and provincial elections, the Social Political Party espoused a program that condemned anti-Semitism and called for the establishment of universal manhood suffrage, reform of housing through increased governmental regulation, extension of social insurance coverage, an eight-hour workday in factores, unrestricted union activity, public ownership of utilities, improvement of welfare programs, establishment of producers' and consumers' cooperatives, and maintenance of secular public schooling.

Although the party had the support of a number of intellectuals of considerable prestige, such as Professor Eugen von Philippovich and Michael Hainisch, it failed to secure significant political power. At the peak of its electoral success in 1898,

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the party had only six elected representatives: one in parliament, three in the Lower Austrian diet, and two in the Viennese municipal council. Moreover, the debilitating atmosphere of partisan obstructionism that prevailed in Austrian political assemblies at the time virtually precluded any constructive contribution by such a miniscule party. After electoral defeats in 1900 the party tried to keep itself alive by forming alliances with its earlier rival, the Liberal Party. The attempt did not succeed, however, and the driving force behind the party, the Social Political Club, dissolved itself in 1907.

Why did the movement fail so utterly? In large part because, for all its espousal of democratic principles, the Social Political Party was an elitist group of notables, an *Honoratiorenpartei*. It had no permanent party officials and its candidates disdained opn campaigning. Following this orientation, it could never gain a mass following and compete with Christian Socialism or Social Democracy. Unwillingness to adopt modern political practices condemned the fledgling party to practical impotence.

The program and organization of the social political movement were strongly influenced by the Fabians in England; indeed, the initial forerunner of the party was called the *Fabier Gesellschaft*. Like the Fabians, the effect of the Social Political Party in Austria was more indirect than direct. The party's approach to the social and economic problems of industrial capitalism anticipated many aspects of the modern welfare state. Since the movement's real significance is to be found in this intellectual heritage rather than practical political activity, it is a great pity that the reader is given so little of this side. Although Holleis alludes to this future influence, the bulk of her book is a bloodless narrative of the dreary story of electoral failure.

WILLIAM H. HUBBARD
Concordia University

C. EARL EDMONDSON. *The Heimwehr and Austrian Politics, 1918–1936.* Athens: University of Georgia Press. 1978. Pp. x, 352. $22.00.

The currents and crosscurrents in the Austrian political kaleidoscope during the interwar years are so complex that any historian undertaking to bring order out of this chaos should be congratulated on his courage alone. If in addition he succeeds in "making a mesh out of a mess" as nicely as C. Earl Edmondson has succeeded, he deserves great credit indeed. The relationships between the Heimwehr and the driverse right-of-center political groups are not easy to disentangle, any more than the Heimwehr's internal contradictions and centrifugal forces are easy to focus on center. The author has managed to do just that, with a steady hand and an objectivity befitting the scholar. The heart of the book, following the normally anticipated introductory chapters (1918–27), covers the nine-year period between the tragedy of July 1927, which presaged the greater disasters of 1934, and the demise of the Heimwehr in October 1936.

It may be supposed that the educated layman will find the volume both interesting and instructive, whereas for the "initiated" it will be more interesting than able to provide much new information. All the same, the specialist will also appreciate this book. Its treatment of the intricate, often equivocal, liaison between the Heimwehr and the Dollfuss/Schuschnigg regimes and the conglomeration of the various antisocialist factions is very sharp indeed. Edmondson's description of the Heimwehr's climb to power, following the fiasco of 1931, of its ascension to some kind of "summit," such as it was, and of its downward slide between 1934 and 1936 belongs to the most dramatic historical narratives in the saga spanning the decade between the Palace of Justice fire and the fall of the Heimwehr. Edmondson shows clearly, in a lucid and eminently readable manner, how the Heimwehr was vainly groping for an identity while chafing under a lack of a good leader who might have "invented" such an identity in the teeth of the "real facts." The author's year-by-year narrative never sinks to the level of mere chronicle. He organizes the bewildering political realities in a principled and highly articulate way.

The development of the Heimwehr, especially after 1927, with the organism of the confusion that was Austria, can only be understood by laying bare the tissues of the whole organism, particularly its nonsocialist part. This means that the complicated and finely spun connections between the clerical-conservative and the national orientations, between the corporative and the laissez-faire capitalistic factions, between the pro-Nazi and the anti-Nazi lobbies, between the pro-Mussolini and the anti-Mussolini pressure groups within the Heimwehr must all be patiently opened up to public view. The task is formidable, to say the least, but Edmondson is equal to it.

This book may not be the pioneer performance one always hopes for, but it must be conceded to have made its main points with real competence and in a style that is a pleasure to read.

ROBERT SCHWARZ
Florida Atlantic University

CHRISTINE MEEK. *Lucca, 1369–1400: Politics and Society in an Early Renaissance City-State.* (Oxford Historical Monographs.) New York: Oxford University Press. 1978. Pp. viii, 424. $31.00.

In this solid study, Christine Meek examines the political development of Lucca in Tuscany from 1369,

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when the city regained its freedom from Pisan rule, until 1400, when one of its principal citizens, Paolo Guinigi, established an open despotism over it. Through all this period, Lucca remained a small, even backward town, struggling to keep its freedom against the ambitions of powerful neighbors—Florence, Milan, Pisa. The attraction of its history rests in large part on its well-preserved archives, which Meek has clearly plumbed to their depths. Its interest too is the light these archives offer in regard to that hoary problem of Renaissance historiography: why did the republican communes of the Middle Ages give way to the despotisms of the Renaissance?

In responding to this question, Meek has something to say about most aspects of Lucchese history from 1369 (or even before) until 1400. She considers the economic basis of the Lucchese state—its territory, population in city and countryside, industrial and commercial wealth, rural resources, and the tax system designed to tap them. She pays close attention to external relations, stressing that the Lucchese wanted only to be left undisturbed in the jungle of Renaissance politics. And she finally examines the factions or parties within the city—the Guinigi, the ultimate victors, and their rivals the Forteguerra. She finds no class interests aggravating their rivalry and discerns almost no participation of the humblest levels of society, the *popolo minuto*, in the political life of the city. Both great factions were birds of the same feather; they were both dominated by great families, recruiting supporters among the urban poor and even the peasants by patronage and favors. Their partisans looked only for personal advantage and gave no thought at all to the interests of any class or economic group.

Why did the Guinigi despotism come to prevail at Lucca in 1400? According to Meek, the seizure of power represented primarily an "act of will" on the part of Paolo Guinigi and his partisans. They seized absolute power in Lucca, we are asked to believe, because they wanted to.

It is possible to acknowedge Meek's long and diligent labors in the archives, to recognize the value of her many discoveries, and yet to concede in the final analysis that her book has no thesis. She has compiled an aggregation of informative, well-documented, well-informed essays on various aspects of fourteenth-century Lucchese history. These essays will be, and ought to be, read with interest and appreciation. But they do not hang together, or she at least does not bring them together. To argue that Paolo Guinigi established his *signoria* over Lucca because this was his will is no more than a tautology; she never really confronts the question that her book was supposed to answer.

DAVID HERLIHY
Harvard University

SHLOMO SIMONSOHN. *History of the Jews in the Duchy of Mantua.* (Publications of the Diaspora Research Institute, number 17.) New York: KTAV Publishing. 1977. Pp. xvii, 902. $35.00.

The publication of the original Hebrew version of this book some fifteen years ago represented a milestone for research in Italian Jewish history in particular and in the history of European Jewish communities in general. Unlike those of other Jewish communities, most of the Jewish community archives of Mantua have miraculously survived along with the numerous documents concerning Jews in the Gonzaga archives as well as a large collection of Hebrew manuscripts and printed books, products of the rich cultural life of Mantuan Jewry. The author exploited all of these treasures to produce one of the most exhaustive treatments of a single Jewish community from the fourteenth to the eighteenth centuries. Simonsohn's achievement was especially impressive in its detailed portrayal of the internal structure of a flourishing Jewish community—its administration of taxes and welfare, its social life, and its manifold relationships with other Jewish communities. And even more importantly, Simonsohn's work was particularly valuable, as he correctly indicated, as "a cornerstone for further extensive research," equipped with useful indexes, thumbnail biographies of Mantuan Jews, and copious notes on numerous details of Jewish communal activities.

The appearance of the English version of this book is accordingly most welcome, for now its impressive scholarship will be available to a larger audience of researchers, particularly those working in the Italian Renaissance. It is unfortunate, however, that the author has not fully utilized the opportunity of a new edition to correct some of the deficiencies of the original version and to update his findings in light of the last fifteen years of scholarship in Italian and Jewish history.

Simonsohn's book is far too long; there is too much explication of texts and little economy of words. There remains a great need to synthesize many tedious details that often overwhelm the reader. Thoroughness in this case does not necessarily yield greater understanding. Instead of mentioning, for example, every banker and seemingly every article of each important loan contract, might not the author have sharpened his analysis of Jewish economic life by integrating his rich data into a larger picture of the political needs of the state, the economic life of its citizenry, and the oppressive problem of poverty, as Brian Pullan has done for Venice? Simonsohn's cultural history is similarly lacking. Instead of cataloguing names, books, and authors, the author might have related more profoundly the fascinating intellectual endeavors of

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Mantuan Jewry to cultural developments at the Gonzaga court and elsewhere. To speak generally of the influence of "the spirit of the Renaissance" on the Jewish community is too little indeed. Instead, this reader would like to know precisely how Renaissance culture influenced the values and cultural priorities of Mantuan Jews and why their "Renaissance" interests endured long after the Renaissance ended.

In fairness to Simonsohn, the state of scholarship on Italian Jewry fifteen years ago was as yet underdeveloped; sources still needed to be described before more critical analysis was possible. In many respects, this is still true today; but certainly research on Italian Jewry has made tremendous strides since the early 1960s—much of it conducted by Simonsohn's own students. More is now known on other Italian Jewish communities, on the institution of the rabbinate, on cultural developments, and on papal policy toward Jewry, to mention only a few examples. Even more impressive are the exciting advances made by Renaissance historians in investigating popular religious behavior, poverty, elites, and family and corporate relationships. Have we not reached the time when some of the imaginative insights and methodological advances in the study of Italian Christian culture might be applied with some measure of success to the Jewish data so readily available in a work such as Simonsohn's?

The English edition is also flawed by too many inconsistent spellings (for example, Leghorn and Livorno, Paul and Paulo); incorrect or inconsistent transliterations (for example, Sarfati but also Tsahut); use of undefined terms (*Tossafist*, *massari*); and, especially, inappropriate or awkward translations ("cruel bill," "evil decree," "yoke exceedingly heavy"). It is a pity that this valuable work was not prepared with more care in such matters.

DAVID RUDERMAN
University of Maryland

VOLKER HUNECKE. *Arbeiterschaft und Industrielle Revolution in Mailand, 1859–1892: Zur Entstehungsgeschichte der italienischen Industrie und Arbeiterbewegung.* (Kritische Studien zur Geschichtswissenschaft, number 29.) Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 330. DM 58.

This is an important and original contribution on the growth of Milanese industry and the formation of the working class in Milan between 1859 and 1892. Volker Hunecke's volume follows classic studies of Werner Sombart, published over eighty years ago, on the rise of the Italian working class and on wages and strikes in Italy. Hunecke begins his monograph with a review of recent theories of Italy's industrial revolution. The most original parts of the book, however, deal with demographic developments and urbanization; the growth of local industry and its spread to the suburbs (Corpi Santi); changing patterns of residential and industrial employment in Milan; the evolution of wages, working conditions, education, health, literacy, and family budgets; the relations between the public authority and Milanese working-class organizations; and an analysis of strike activity by Milanese workers between 1860 and 1892. The author has mapped the transition of the working class of Milan from its infancy as loosely organized mutual-aid societies, led by upper-class patricians, to its maturity as autonomous, class-conscious, militant, working-class organizations such as the *Partito Operaio Italiano* and the *Partito dei Lavatori* that emerged in the 1890s.

Hunecke argues that there is a uniquely Italian way to industrialization based on a highly labor-intensive system of production, low marginal productivity of labor (with generous recourse to unskilled female and child labor), worker exploitation (through strict neutrality of the public power in relations between labor and capital), small-sized plants, and so on. Italian industrialization, he points out, has been slow, discontinuous, and decentralized, and this has profoundly influenced the course and development of the Italian labor movement and has made its history unique. Strike activity among Milanese workers, he shows, was issue-oriented, largely independent of central control, and relatively immune to ideology. Yet we would like to know more about the activity of another group of workers in order to draw the relevant conclusions.

Hunecke's analysis of Italian economic development brings it very close to the labor-surplus model of Arthur Lewis. Development in an economy with an unlimited supply of unskilled, low-wage, low-productivity labor, and obsolete technology depends, according to Lewis, on increased investment and higher wages for growth. In Italy, growth was delayed because the state and the entrepreneurial class followed anti-industrial policies and were totally disinterested in the development of a supply of skilled labor. Milanese industrialists maintained a tradition of philanthropy and paternalism with respect to their workers—even into the industrial age—because of a deep-seated fear of an urban proletarian class that they believed would upset the existing social structure and political relationships. They therefore blocked reformist solutions of social and economic questions. Italy thus remained socially and politically underdeveloped and was one of the last states in Europe to recognize workers' rights—the right to organize, to negotiate minimum wages and hours, and to regulate conditions of work.

Hunecke also believes that Italian industrial-

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ization must be understood as a regional phenomenon that began in the North (Milan) in the 1870s and 1880s, or even earlier (in the 1830s and 1840s), as in France. The Depretis expansion and Giolittian boom are, therefore, continuations of a longer cycle of growth that began in the early nineteenth century. Armando Sapori, Gian Maria Bravo, and Stefano Merli have called attention to the fact that a factory system and an urban proletarian class were already visible in the industrial centers of northern Italy at the time of unification (1861). The significance of this point, however, for Italian historians has not yet been fully grasped.

Hunecke has made an important, informative, and original contribution. His study should serve as a model for future work in labor history by historical sociologists and sociologically minded historians. It is well researched; his sources have been used with ingenuity; and his bibliography—barring a few surprising omissions—is complete.

IRA A. GLAZIER
Temple University

PAOLO PEZZINO. *La riforma agraria in Calabria: Intervento pubblico e dinamica sociale in un'area del Mezzogiorno, 1950/1970.* (Istituto Nazionale per la Storia del Movimento de Liberazione in Italia. Saggi e Biografie, number 394.) Milan: Feltrinelli. 1977. Pp. 253. L. 8,500.

This sociological study of contemporary Italy is clear, focused, and persuasive. Paolo Pezzino starts with a brief examination of the immediate postwar land occupations and other political movements of Italian agriculturalists. (He is careful to delineate the groups he discusses by their status vis-à-vis land use and ownership, for he correctly sees that owners of land inadequate to support themselves, *mezzadri* and other share contract farmers, and wage-earning proletarians are all disadvantaged by competition from capitalist agriculture, but each disadvantaged in different ways.) He proceeds to a critical analysis of the positions of Italian political parties on agricultural reform. His most thoroughgoing critiques are devoted to liberal and Communist Party positions, both of which tended to subdue partisan disagreement in the immediate postwar years, in keeping with the spirit of the Resistance. For the Communists, the result was a policy that was anti-Fascist rather than anticapitalist.

He reviews the emergence—through pressure from strikes and land occupations on the one hand and through legislation on the other—of the Italian land reform program. Pezzino concludes from this relatively quick review that there was a failed revolution at the end of 1945 and in early 1946, during which time the left missed the significance of *mezzadri* protest, with serious consequences for later reform (p. 31). He then examines the legislation itself, which he interprets as an effort to reduce political pressure on the regime by promoting small holdings for landless laborers but, at the same time, not increasing the size of holdings of those who already owned land.

How the legislation worked in practice is a different story, however, a story documented for Calabria only, in the rest of Pezzino's study. The conceptual perspective that he adopts is that of a dual labor market, and he follows Massimo Paci (*Mercato del lavoro e classi sociali in Italia* [1973]) in emphasizing a "marginality/capitalist development nexus" and describing changing patterns of social stratification in the countryside (p. 33).

Pezzino carefully lays out and criticizes the sources he has used. The detailed records of the Sila land reform agency and the *Cassa del Mezzogiorno* (southern development fund) were not open to him, so he used other official sources, which provided primarily aggregate statistics. These sources included, however, the individual schedules of a survey of landholding around 1950, cadasters, and population and industrial censuses. Unfortunately, each source used somewhat different socioeconomic categories to group individuals. His strategy was to regroup into as uniform categories as possible for Calabria as a whole and then to examine in greater detail four communes with distinct ecological and economic characteristics.

He concludes that the effect of land reform legislation has been limited and uneven; some communes have benefited more than others. The crucial failures have been that (1) large landowning has continued; (2) viable, small, self-supporting farms have not become the modal form of landholding; (3) although there are fewer wage-earning agricultural laborers, increased numbers of urban industrial workers in "precarious" occupations (primarily construction work) have appeared. The land reform program, Pezzino argues, has not succeeded in building a prosperous base for out-migration and hence for reduced population, or in reconstructing the agrarian sector. Instead there has been internal migration to weak alternative economic sectors. The hopes of liberal reformers, such as Manlio Rossi-Doria, have not been realized, Pezzino argues, because the reform was aimed simply at part of the capitalist economy and unreformed sectors undermined the program.

Pezzino's argument is well documented on the aggregate level, but more detailed studies (which, disappointingly, his communal case histories do not provide) are needed before his case is proved. It is also disappointing to a historian, moreover, that he did not interview the inhabitants of the villages to see what the new stratification means in human

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terms—to the new holders of small farms, to the new urban proletarians, or to the large landlords, still holders of the vast majority of agricultural acreage. After all, as a student of the recent past, Pezzino has a source that most historians lack, the actors themselves.

LOUISE A. TILLY
University of Michigan

ALFREDO BRECCIA. *Jugoslavia, 1939–1941: Diplomazia della neutralità.* (Università di Roma, Facoltà di Scienze Politiche, number 25.) Milan: Giuffrè Editore. 1978. Pp. xiv, 811. L. 18,000.

In the interpretation of any controversial episode or period in diplomatic history there comes a time when something close to a definitive study can be done. It may require a thirty-year wait for the availability of official documents and the full range of memoir material. All that is needed, then, is a historian with sufficient industry, skill, and feel for his subject to put it together. Alfredo Breccia, of the universities of Messina and Rome, has performed this role for the tangled question of Yugoslavia's attempt to maintain its neutrality from the outbreak of the Second World War in September 1939 to the invasion and partition of the country by the Axis powers in April 1941. Making full use of British, German, and especially Italian material, piecing together the Soviet side from indirect sources, and drawing on the research of Yugoslav historians, he gives us a masterful account that is rich in detail without losing track of the threads of policy.

The warring powers pursued their deadly competition and others not yet involved, like Italy and Russia, sought advantage, each trying to subvert, support, or otherwise influence the position of Yugoslavia as the case may be; Breccia follows, against the broader background of the war, their every significant move in his meticulous case study of interacting policies in this one region. One of the striking features of his narrative is how uncertain and unpredictable many of those moves were. There was an absence of clear alignments and commitments on the part of the powers in this early period of the conflict, a *sacro egoismo* that marked not only Italy's policy but those of all the others as well. Thus we find tension and competition between Germany and Italy despite the "pact of steel," between Germany and Russia despite the agreements of 1939, and between Britain and France (before June 1940) despite their joint commitment to the war. These currents and crosscurrents gave the Yugoslav leaders more room for maneuver than they had a right to expect. They took advantage of it, but the country was too exposed, too weak, and too divided within itself to be able to make neutrality last indefinitely.

The guiding hand in the Belgrade government was that of the regent, Prince Paul, and it is one of the merits of Breccia's effort that he subjects Paul's policies and diplomacy to searching and objective investigation. His conclusions tend to confirm the judgments reached by the late Jacob B. Hoptner in his pioneer work, *Yugoslavia in Crisis, 1934–1941* (1963), discounted by many at the time as an apologia for Paul. There are many points on which Breccia sheds new light, not least of them the shadowy series of events leading up to the coup d'état of March 27, 1941, the subject of violent polemics ever since it happened. Here he is quite severe with the British, for the part they played in those events, and with British historians, now that the facts are clear, for not admitting it. The revolution in which Yugoslavia "found its soul," to use Churchill's words, was in fact London's answer to Prince Paul's finally yielding to intolerable German pressure to sign the Tripartite Pact; it was, as well, a deliberate British decision to extend the war to Yugoslavia since Hitler was then certain to attack. It may have been right for Britain, and it was probably right for Russia as things turned out, but it was suicidal for Yugoslavia, as Prince Paul foresaw.

JOHN C. CAMPBELL
Cohasset, Massachusetts

ŞERBAN RĂDULESCU-ZONER. *România şi Tripla Alianţă la începutul secolului al XX-lea, 1900–1914* [Rumania and the Triple Alliance at the Beginning of the Twentieth Century, 1900–14]. Bucharest: Editura Litera. 1977. Pp. 187. 24 L.

Paul W. Schroeder has argued that Rumania's defection from the Triple Alliance on the eve of the First World War, rather than the Serbian menace, was the decisive blow to Austria's great power status and that the realization of this in Vienna, more than the assassination of Archduke Franz Ferdinand, was the determining factor in Austria's decision to resort to force in 1914. But whether one accepts this or not, the history and causes of Rumania's estrangement from Germany and Austria-Hungary are important and form the subject of the book under review.

The author, a senior researcher at the Institute of History in Bucharest, believes that prior to 1909 Rumania's commitment to the Triple Alliance was essentially secure. Germany supplied Rumania's two most urgent needs: political stability (threatened by Russia) and capital (essential for its developing economy). But these centripetal forces were increasingly balanced by the centrifugal influences of the internal and foreign policies of Austria-Hungary. In 1909, the Hungarian government stepped up its policy of racial chauvinism directed against

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the three million Rumanians living in Transylvania. This policy of "Magyarization" enflamed Rumanian irredentism, and the advent of Ion I. C. Brătianu as head of the Liberal Party and as premier (1908–10) brought to power in Bucharest a man determined to utilize the first favorable opportunity to annex Transylvania and complete the unification of the Rumanian people. Şerban Rădulescu-Zoner presents evidence to show that, although Brătianu was careful not to estrange Rumania's allies, he covertly began to encourage irredentism in the Old Kingdom and among the moderates of the Rumanian National Party in Translyvania while at the same time cultivating interest in Rumania on the part of the Triple Entente, whose recent consolidation provided Rumania with a viable diplomatic alternative.

At the same time, the Dual Monarchy, alarmed by the South Slav threat and Russia's involvement in it, embarked on a more activist policy in the Balkans, symbolized by the annexation of Bosnia in 1908. Vienna's efforts to prevent the growth of a Greater Serbia and later to promote the aggrandizement of Bulgaria as a counterweight were viewed with alarm in Bucharest, where the maintenance of the status quo in the Balkans was considered to be the cornerstone of Rumanian foreign policy.

Rădulescu-Zoner argues that, after the Bosnian Crisis, Rumania, vis-à-vis the Triple Alliance, was militarily "unreliable" and its attitude one of "nonconformism." Why then did the formal contours of Rumanian foreign policy remain unchanged? Because, he answers, Rumania was growing even more dependent upon German capital, especially to meet budget deficits and to finance the developing petroleum industry, and because of the increasingly unstable political situation in the Balkans (the decline of Turkey, Austro-Russian rivalry), which "placed in danger the territorial integrity of the nation." Although public and official tensions with Austria increased, Rumania had no quarrel with Germany; the alliance was renewed in 1908 and in 1912 because Bucharest recognized that "the road to Berlin ran through Vienna."

But, according to Rădulescu-Zoner, a decisive "reorientation" of Rumanian foreign policy took place during the Balkan Wars. Feeling threatened by the impending change in the status quo, expecially the enlargement of Bulgaria, Rumania loyally supported the policy of the Triple Alliance during the first conflict (1912) only to find that, during and after the Second Balkan War (1913), Austria disregarded Rumania's vital interests and threw its support to Bulgaria. This, the author believes, was the crucial turning point. Rumania now began to "disengage" actively from its thirty-year connection with Germany and Austria-Hungary and to "reorient" its foreign policy toward the Triple Entente. The architect of this new policy was Brătianu who, returning to power in January, 1914, skillfully averted the wrath of Berlin and Vienna while he effected a rapprochement with Russia and its allies.

Rădulescu-Zoner's use of archival sources, chiefly Austrian, gives his treatment detail and depth lacking in previous attempts to cover essentially the same ground. Taken together with Vasile Vesa's *România şi Franţa la începutul secolului al XX-lea 1900–1916* (1975), this book provides the best survey of Rumania's prewar foreign policy currently available.

GLENN E. TORREY
Emporia State University

MIHÁLY BUCSAY. *Der Protestantismus in Ungarn, 1521–1978: Ungarns Reformationskirchen in Geschichte und Gegenwart.* Part 1, *Im Zeitalter der Reformation, Gegenreformation und katholischen Reform.* (Studien und Texte zur Kirchengeschichte und Geschichte, First Series, number 3, part 1.) Vienna: Hermann Böhlaus Nachf. 1977. Pp. xix, 303. DM 76.

Within the lands of the Crown of St. Stephen, Protestantism has had a rich and varied history. First accepted by the German citizenry of the cities of upper and western Hungary and of Transylvania, the reformist doctrines emanating from Germany and Switzerland very quickly found ready and staunch adherents among the diverse peoples and classes of Hungary. Although the fortunes of the Protestant movements waned in the face of a determined and forceful Counter Reformation and the absolutist policies of the Habsburgs of the seventeenth century, the Protestants of Hungary were never completely eliminated as were their coreligionists in Bohemia. They continued to exercise an influence upon the cultural and political life of the nation far in excess of that warranted by their number.

Nevertheless, until quite recently, the history of Protestantism in Hungary has not received the attention due it from most historians in Western Europe or the Americas, in part due to the problems of language. This makes especially welcome the appearance of this volume by Mihály Bucsay, the doyen of Protestant church historians in Hungary. The first of two volumes to be produced under the sponsorship of the Institute for Protestant Church History in Vienna, this survey covers the period of the Reformation and Counter Reformation in Hungary from the disaster at Mohács (1526) until the peace of Szathmár and the Diet of 1714, which terminated the legal rights of the Protestant churches to toleration. It also includes a masterful, brief survey of the church in Hungary since the

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tenth century and contains an extensive bibliography of some sixty-nine pages, which, despite the awkward use of abbreviations, is one of the best listings of scholarly material available for the study of Hungarian church history.

Bucsay has not contented himself with merely revising his earlier, brief, single-volume study on the same topic but has thoroughly restructured it and expanded it, incoporating the most recent findings of scholars from around the world. While he himself belongs to the Hungarian Reformed tradition, Bucsay does not neglect the Lutheran, Anabaptist, or Antitrinitarian reformers or developments among the non-Magyar inhabitants of the "Apostolic Kingdom." His presentation is clear and concise and devoid of the religious or political polemics that have so often marred earlier studies. Although there will be those who cannot agree fully with his conclusions concerning individual reformers or with his emphases, this work seems destined to become a standard introduction to the rich traditions and literature of the early history of Protestantism in Hungary.

DAVID P. DANIEL
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MIKLÓS MOLNÁR. *A Short History of the Hungarian Communist Party.* Boulder, Colo.: Westview Press, and Dawson, Folkestone, Kent. 1978. Pp. 168. $16.50.

Although its title is reminiscent of an official publication, this monograph is written by an eminent Western scholar of Communist affairs. Budapest has yet to consecrate a volume solely to party history. Miklós Molnár's book makes evident the reasons for this reluctance.

Molnár claims that the Communist Party of Hungary (HCP) from 1919—a year after its inception—to 1945 was "miniscule." Following the collapse of the Soviet Republic in August 1919, it consisted of émigrés, residing mostly in Moscow. Having "no relation with the realities and forces in Hungarian society" (p. 30), it was not a working-class party and remains "an anomalous party without real social cohesion" (p. 112). Whereas official publications boast of the party's success after World War II, the author attributes this largely to the Soviet Union's actions. These gave the party a monopoly of power without a popular mandate and signaled a change in party fortunes. Its subsequent growth into a mass party represents a break in its history, making its pre-1945 heritage almost irrelevant.

Molnár emphasizes post-World War II developments. Most of the party's pre-1945 history is concentrated in the first chapter. His examination of this period is superficial and often imprecise. Insufficient research and an overreliance on published sources from Communist Hungary weaken the essay. Archival research mentioned in the preface is not indicated in the text. The role of the HCP during the Károlyi regime is exaggerated, as are the membership figures. Discord in the united Socialist-Communist party during Béla Kun's Soviet Republic is glossed over. The factional disputes between party builders and liquidators in the exiled HCP are underemphasized in official historiography. These conflicts of the early twenties would reflect badly on those who are now members of the Communist pantheon. Unfortunately, in his discussion Molnár merely repeats what can be learned from recent Hungarian publications. Nor is there satisfactory treatment of the "realities" that made the party and its ideology so unpopular in Hungary. Horthyite persecution is only partially responsible. The preface indicates that the HCP played an important role in the international movement. This thesis, however, is not satisfactorily explored. The reorganization of the Comintern and the *Twenty One Conditions*—partially responses to failures in Hungary—are not considered.

Although the pre-1945 narrative is weak, it is followed by some challenging interpretations in the remainder of the book. The author rejects the "blueprint theory" (p. 113) of the seizure of power in Hungary. He suggests that the HCP was a sincere participant in front politics and was willing to wait for a legitimate transfer of power. This interpretation stresses the national component of HCP policy, a trend that changed in 1948 on Stalin's cue. From 1953 to 1955, during Imre Nagy's reforms, there was a return to national policy. The aims of the Hungarian Revolution of 1956, which outpaced Nagy's, have not been achieved by Kádár as has been argued elsewhere. Molnár, however, considers that Kádár is building on the reforms initiated by Nagy. Despite Kádár's success, the book's conclusion reminds the reader that the party lacks popular legitimacy. Applying Annie Kriegel's model for the French party to the HCP, Molnár argues that it can also be viewed as a "counter society" (p. 143). Basing its power on abstract legitimacy, it has created no new value system, and its *raison d'être* is bureaucratic power.

The author presents a fine short history of the HCP after 1945. An acceptable complete history is yet to be published.

PETER PASTOR
Montclair State College

MARCELI KOSMAN. *Protestanci i kontrreformacja: Z dziejów tolerancji w Rzeczypospolitej XVI–XVIII wieku* [Protestants and the Counter Reformation: From the His-

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tory of Toleration in the Polish Commonwealth from the Sixteenth to the Eighteenth Centuries]. Summary in German. Wrocław: Ossolineum. 1978. Pp. 172. 42 Zł.

Marceli Kosman is one of several scholars who recently has been publishing prolifically in a field long closed to Polish researchers—Lithuania during the union with Poland. Kosman has worked extensively in Lithuanian archives and draws upon contemporary Lithuanian and other Soviet scholarship. Polish and Western readers should be grateful to him for making this material accessible.

One of Kosman's books, on the decline of Lithuanian paganism, was reviewed here last year (*AHR*, 83 [1978]: 227–28). The strengths and weaknesses of that work are shared by the book now under consideration. Notwithstanding the title's broader implications, *Protestanci i kontrreformacja* deals primarily with Lithuania. As before, Kosman has discovered valuable material. He offers new data to support the already fairly familiar view that the Lithuanian and Polish Reformations were largely the work of noble patrons who reformed their parishes, promoted Protestant education, and strove for the toleration of religious minorities in the face of growing fanaticism among Counter Reformation clergy, courtiers, and urban mobs. Particularly important is Kosman's research into the Lithuanian guilds' vigorous Catholicism.

By comparison with some other recent scholarship on the Counter Reformation's appeal to artisans, however, Kosman's findings seem a little superficial. He hesitates to go beyond pointing out the guilds' continued adherence to old devotional rituals, and he explains urban popular Catholicism chiefly in terms of artisan antipathy to Protestant foreigners and nobles. Although I grant that the sources are not as extensive as they are for West European towns, my appetite has been whetted for more. Other difficulties arise when Kosman describes Lithuanian religious consciousness from the perspective of the comparative study of religion. Despite early promises, he never draws systematic comparisons with other Reformation movements, except when making such generalizations as "baroque Catholicism spoke more to the emotions than to the intellect" (p. 34). Kosman rightly insists that the Lithuanian Protestant magnates had no economic motive for attacking the old churches; both the Catholic and the Orthodox ecclesiastical organizations were poorly endowed. Why, then, did the Reformation appeal to so many, and why did some even turn to Arianism? Why did other nobles remain Catholic? Why did the Reformation arouse so little interest among the Orthodox? Was it mere opportunism that caused many magnate families to split into Protestant and Catholic branches and tolerate mixed marriages in which children followed the religion of the parent of their sex? Kosman's suggestion that these attitudes resulted from the Lithuanians' recent conversion from paganism is not wholly convincing; the Polish nobility, which had been Christianized for five hundred years, behaved similarly. Kosman seems determined to avoid confronting the possibility that at least some nobles actually believed the doctrines they professed.

Kosman's promised further publications potentially can be of great interest to Westerners. It is to be hoped that he will deepen his analysis of the nobles' unstable balance of religious and political ideals and his study of popular religious mentalities.

JAMES MILLER
Stockton State College

HENRYK PIASECKI. *Żydowska organizacja PPS, 1893–1907* [The Jewish Organization of the PPS, 1893–1907]. (Żydowski Instytut Historyczny.) Wrocław: Ossolineum. 1978. Pp. 273. 40 Zł.

The last fifteen years in Poland have not been conducive to serious research in Jewish studies. The anti-Zionist campaign led to the nearly total dismantling of Jewish research facilities, the emigration of nearly all capable scholars, and the takeover of the field by Communist Party ideological watchdogs, secret policemen, careerists, and even reputed former fascist activists. Today, consequently, very few legitimate scholars pursue their interests in the Jewish field. Henryk Piasecki is a case in point: an older gentleman, long-retired from a government agency, and an amateur historian, whose insight into the subject comes from rich personal experience (mostly in the Communist movement). And yet, one cannot but appreciate the fruit of his painstaking research, which led to a valuable book.

Piasecki discusses a very little known chapter in the history of the Jewish socialist movement in Poland, namely the origins and activities of the Jewish Organization of the Polish Socialist Party (PPS). Unlike the individual Jews in the PPS—assimilated, highly educated, active in the socialist movement since its inception, and contributing their theoretical skills—the members of the Jewish Organization were genuinely representative of the Jewish masses, a fact demonstrated by their occupational characteristics (workers and craftsmen), their cultivation of the Yddish tongue and Jewish culture, and by their conviction that the struggle for socialism and Poland's independence had to be linked with the improvement of the status of the Jewish population.

Although a distinct group within the PPS, the Jewish Organization had a very limited autonomy,

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and its activities were closely intertwined with other political groupings of Jewish, non-Jewish, socialist, and quasi-socialist orientation. Piasecki has to be credited with presenting a vivid picture of this. He also offers plausible explanations for the limited successes and, consequently, the demise of the Jewish Organization: the failure of the PPS leadership to grasp the importance of multifaceted activities among the Jews (including the full use of the Yiddish language); assimilationist tendencies among the PPS leaders of Jewish descent; the slow evolution of the PPS tactics in regard to the Jewish question; ideological conflicts within the PPS leadership; conflicts between the PPS and the Jewish Organization; and failure of the Jewish Organization to join early in the organization of armed self-defense against pogroms.

It has to be noted, too, that, although Piasecki made full use of the limited materials available in Polish archives, his book is not devoid of weaknesses. I fully agree with Jerzy Tomaszewski of Warsaw University that Piasecki's discussion is too vaguely linked with the program and ideology of the PPS as well as with the radical shift in the development of the Jewish national consciousness in Poland at the time; the failure to discuss these points obscures some of his arguments. Also the book lacks an index and bibliography. Fortunately, the reader is generously compensated by highly informative footnotes, which frequently contain very useful and otherwise unavailable information. Regardless of the limitations mentioned, the book will be of considerable value to scholars in the field.

WLODZIMIERZ ROZENBAUM
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WITOLD BIEGAŃSKI *et al. Polacy w ruchu oporu narodów Europy, 1939–1945* [The Poles in the Resistance Movement of the Peoples of Europe, 1939–45]. Warsaw: Państwowe Wydawnictwo Naukowe. 1977. Pp. 661. 140 Zł.

Polacy w ruchu oporu narodów Europy, 1939–1945 signals one of the first attempts to present a comprehensive picture of the contribution of Poles to the Resistance movement in Nazi-occupied Europe during World War II. The work is largely based on the previous writings of the authors on the subject and contains four parts dealing with the role of Poles in the underground movements in the countries of Western, Southern, and Central Europe and in the areas of Czechoslovakia and the Soviet Union adjacent to Poland. Each opens with a summary of the political and military situation in the country in question followed by facts and figures on the Polish involvement.

In their archival research, the authors were confined to Polish and East European government, party, and military sources and had no access to Western and Polish émigré documents, which, as they themselves admit, tends to decrease the value of the monograph. Consequently, the material collected in the volume is of uneven character and, to a considerable extent, fragmentary. Whereas Polish participation in the Resistance movement in France and Czechoslovakia and the anti-Nazi activities of the Polish military interned in Rumania and Hungary are clearly delineated and profusely documented, the book has little to add to the sparse data on the Polish contribution to the struggle against the German occupation force in Greece, Holland, and Norway.

Marxist in their orientation, the authors place conspicuous emphasis on the role of Communists as organizers of the Resistance movement. They maintain that two distinctive political trends existed in the anti-Nazi movement in wartime Europe: one, "left-wing, revolutionary and progressive" and the other, connected with the Anglo-Saxon Allies and governments-in-exile in the West (p. 541). Nevertheless, the monograph offers a generally balanced and nonbiased view of the underground that operated under the control of the exiled Polish government in London, namely, AK *(Armija Krajowa)*, as well as of the Polish agents who carried out assignments of the Allied Command in occupied Europe.

Noteworthy is the treatment of the Polish Resistance in the western areas of the Ukraine and Belorussia and in the Vilno area, that is, in the former Polish territories annexed by the Soviet Union in the wake of the 1939 Polish-German war. The authors try to traverse this mine field carefully and mainly limit themselves to describing the individual exploits of Polish nationals who fought the Germans with the Soviet guerrilla groups and commando units. They mention the activities of AK and BCh *(Bataliony Chłopskie)*, Polish self-defense formations, in those areas. Reference is made, though in passing, to the clashes between Soviet and AK guerrillas and to the appeal of the Communist Party in Belorussia "to disarm the Polish nationalist bands" (pp. 356–57). The operation *Burza* launched by the AK forces in the area in 1943 and 1944, on the eve of the arrival of the Red Army, is described in detail but dismissed as "a political propagandist maneuver" staged by the London Polish government.

The authors have compiled substantial information on the milieu from which the Polish Resistance sprang: the Polish émigré communities in Europe, POWs, and Polish civilians who were deported by the Nazis to assist in their war effort. The monograph contributes solidly to the identification of Polish members of the European Resistance, pro-

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viding life histories and fighting records in many instances. A lengthy list at the end of the volume names over nine hundred Poles who were active in the European Resistance movement.

LEV STERN
New York, New York

M. D. POLUBOIARINOVA. *Russkie liudi v Zolotoi Orde* [Russians in the Golden Horde]. Moscow: Izdatel'stvo "Nauka." 1978. Pp. 130. 65 k.

This first special study of those Russians who lived in or visited the Mongol centers of the Golden Horde during the thirteenth to fifteenth centuries understandably arouses considerable interest. The subject constitutes an important aspect of Rus'-Mongol relations and raises possibilities for genuine comparative analysis. Unfortunately, M. D. Poluboiarinova has neither examined the topic thoroughly nor explored its broader implications. Thus, this work can best be characterized as a useful but limited introduction to a subject that clearly merits further study.

The book consists almost entirely of two long chapters. In the first, the author examines what the written sources tell us about five distinct categories of Russians whose presence in the Horde is mentioned: princes and their ambassadors, clergy, slaves, soldiers, and merchants. The second chapter describes the Russian-made artifacts, primarily religious items and pottery, found in such Mongol centers of the lower and middle Volga as Selitrennoe (Sarai Batu), Tsarevskoe (Sarai Berke), Vodianskoe, Uvek, Berezovka, and Narovchat.

Perhaps the main problem lies in the lack of an adequate conceptual framework. The topic, for example, is not clearly defined. The author never discusses which Russians are to be considered and why and thus treats regular visitors, permanent residents, and chance travelers indiscriminantly. The geographical limits of the Golden Horde are not explained, with the result that the Crimea and northern Caucasus are dismissed in a few pages while the Bulgar and Central Asian centers are omitted entirely. Aside from the assertion that life was bad for all Russians in the Horde and worst of all for Russian slaves, the author does not test any existing hypothesis or advance any interpretation of her own. Contrary to our hopes, the author fails to provide any new insights into the nature of the Russian experience in the Horde and completely ignores the question of how the Russians fared in comparison both with other tributary peoples within the Golden Horde and with tributary peoples in other Mongol khanates. Poluboiarinova's contribution thus lies primarily in having assembled a convenient summary of the written sources and archeological data.

Other, more concrete problems could be cited. In the first chapter, the author treats each category of Russian in a separate section; no effort is made to integrate the evidence or discuss the overall situation of Russians as depicted by the written sources. Poluboiarinova completely avoids the key question of the second chapter, that is, to what extent do Russian artifacts from Mongol sites reflect a Russian presence at these sites? She automatically considers all such finds as irrefutable evidence of a Russian presence. One is also struck by the paucity of Russian finds from many of the sites. The author's contention that archeology shows a Russian presence throughout the Horde is not sustained.

Now that Poluboiarinova has laid the foundation, we can hope that a truly comprehensive, more perceptive, and less parochial history of Russians within the Golden Horde may be written.

THOMAS S. NOONAN
University of Minnesota, Twin Cities

DAVID L. RANSEL, editor. *The Family in Imperial Russia: New Lines of Historical Research.* Urbana: University of Illinois Press. 1978. Pp. 342. $16.50.

This fine collection of articles, ranging from impressionistic essays on court and gentry family culture to densely statistical works on abandonment and migration, is the product of a generation of Russian historians who insist upon asking new questions about society and who demand rigor in the means used to answer them. The literature on family history in the West has exploded in recent years, as David Ransel's intelligent and informative introduction makes clear. Using methods, insights, and concerns derived from or inspired by this literature, some of the contributors to this volume have begun to fill the vast gaps in our knowledge about human experience in prerevolutionary Russian society: marriage, family patterns, sexual relations, abandonment of children and fosterage, illegitimacy, social and geographic mobility, upbringing, childbirth, medicine, and problems of adolescence. Jessica Tovrov, employing content analysis of didactic family literature of the nineteenth century, shows the alternating use of affection and formal training of gentry sons and daughters. Barbara Engel tries to build a case for a natural alliance between mothers and daughters in the radical cause, an interesting notion but not fully convincing, given the thinness of the evidence. Richard Wortman's brief but rich study of "The Russian Empress as Mother" is fascinating and convincing in explaining, among other things, how thoroughly Dagmar of Denmark (Mariia Fedorovna) stamped her personality on the unlucky Nicholas II. Andrejs Pla-

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kans offers a highly specialized, pioneering study of parentless children in Kurland. Peter Czap, utilizing sophisticated methodology and a high degree of precision, opens up the inner world of the village family in a microcosm of two Riazan estates, which reveals much about compulsory marriage under serfdom. Gregory Freeze, adding to his already impressive studies of Orthodox clerical life, looks at the changing family patterns of priests and the environmental base of their well-known, but heretofore little analyzed, "estrangement" from Russian society. Stephen Dunn's piece on folklore is a thoughtful introduction to the literature but says very little about the family itself. An analysis of lullabies by the Soviet researcher Antonina Martynova goes far in helping us to understand the attitudes of fatalism that inhibited "progressive" medical care and childbearing techniques in the villages.

David Ransel's contribution, a very careful and precise study of foundlings and fosterlings and the people closest to them, is exceptionally valuable in its discussion of the relative power of poverty and shame in abandonment decisions, of the size and social vulnerability of the female servant class in Russia, and, in general, of the dark side of sexual and family culture in the years before the revolution. He has pointed the way to and laid some of the foundations for a comprehensive study of a topic that has been begging for years for serious scholarly treatment: the saga of the "homeless children" (*besprizornye*) in Soviet Russia. Samuel Ramer and Nancy Frieden have done much in their treatments of medical care to describe the appalling health conditions that prevailed in the Russian countryside eighty years ago; this is especially important in view of some recent efforts to glamorize the living conditions of the rural masses in that period. Of direct relevance to the political-social foundations of the revolution are the works of Robert E. Johnson and Diane Koenker on labor migration, working-class life patterns, and the problems of the half-peasant, half-proletarian "hyphenated worker." Both are solid and suggestive; and Koenker's has some exciting ideas about generational subculture, workers' style of life, and the political revolutionary implications of all this.

A work of this dimension is bound to have flaws, but it would be senseless to enumerate all the little lapses and minor errors. The biggest fault, in my mind—and one that can be corrected by subsequent scholarship—is the lack of comparative treatment in most of the contributions (Frieden and Koenker are notable exceptions to this). There are also a few places where the analyst does not rise above the arid array of statistics to the level of larger issues. And there are a few gaps in recent scholarship (for example, new Soviet works by Anokhina and Shmeleva on urban anthropology and by Atsarkin on working-class youth). But the final verdict must be that the collection as a whole is a stunning success. It has achieved exactly what it sets out to achieve: opening "new lines of historical research." The magnificent annotated bibliography by Harold Leich and June Pachuta deserves special mention. Historians who believe that history is more than political analysis will welcome this book with open arms.

RICHARD STITES
Georgetown University

WOODFORD MCCLELLAN. *Revolutionary Exiles: The Russians in the First International and the Paris Commune.* Totowa, N.J.: Frank Cass. 1979. Pp. xv, 266. $19.50.

Woodford McClellan's study of the Russian revolutionary involvement in the First International and the Paris Commune brings to light an important aspect of the history of nineteenth-century social movements. McClellan argues, contrary to all previous work on Russian radicalism, that Russian social democracy did not begin in Geneva in 1883 when George Plekhanov founded the Group for the Emancipation of Labor. Instead, he has uncovered material to support the contention that the first Russian social democrats existed during the period 1863–73. These "forgotten ancestors of Plekhanov . . . cooperated with Marx in the International and called him one of their teachers" (p. xiii). Although McClellan concedes their Populist ideological origins, he is convinced that in terms of political orientation, they "stood squarely in the camp of social democracy and therefore of Marxism" (p. xiv).

The other main argument of the book is that the Marx-Bakunin conflict was less instrumental in causing the collapse of the International than was the counteroffensive of the European governments against the left after the Paris Commune. To prove his case, McClellan has utilized published sources as well as materials from numerous archives in Moscow, Amsterdam, Paris, Geneva, and elsewhere. With this evidence, he has constructed a narrative in which he describes in depth the personalities and activities of those original Russian social democrats; a good deal of space is also devoted to Nechaev and Bakunin who, although not social democrats, were clearly involved with the International.

The members of the Russian section of the International were few in number and produced little in the way of theoretical writings. They were primarily activists, not intellectuals. In an age that seemed to specialize in leaflets, pamphlets, and underground journals, the main contribution of this group was a Russian translation of the first volume of Marx's *Capital*. Although they briefly turned a Bakuninist

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paper, *Narodnoe Delo* (*People's Cause*), into a social democratic organ, they did not have a Russian periodical of their own. Nevertheless, as a result of McClellan's work, the radicalism of Nicholas Utin, Herman Lopatin, Elizabeth Tomanovskaya, Anna Jaclard (née Korvin-Krukovskaya), Olga Levashova, and Catherine Barteneva will now become more familar to students of Russian history.

There are some questions about the book that must be raised. First, McClellan nowhere defines what he means by social democrat in the Russian context of this period. Equating the term with a partial or "eclectic" acceptance of Marx's campaigns and concepts does not clarify this ideological designation. McClellan writes that the Russians "basically used the terminology and categories of social democracy" (p. 230), but we do not know whether, and to what degree, they actually read Marx. Second, although he generally manages to remain even-handed in his analysis of the Marx-Bakunin controversy, at times he portrays Bakunin and anarchism unfairly, and leans (perhaps unintentionally) toward the Marxist side of the issues (see especially pp. 14, 55, 97, 189). He even states that Bakunin "never had much standing in Russia" (p. 233), despite Paul Avrich's convincing work on Russian anarchism to the contrary. Third, in the absence of a deeper theoretical or psychological exploration of the nature of the radical emigration and of more information on the émigré community as a whole, McClellan's discussion of the conflicts between Bakunin and both Marx and Utin can too often be reduced to overly simplified personality differences—"Bakunin could not stomach Utin" (p. 16).

McClellan has clearly demonstrated that Marx's influence on Russian revolutionaries antedates the Plekhanov group. Strangely enough, he does not include a sketch of Nicholas Sazonov, who befriended Marx in Paris during the 1840s and was, in all likelihood, the first Russian to become a disciple of Marx. None of these reservations, however, should deter scholars and students from reading this interesting account of an unnecessarily neglected dimension of Russian revolutionary history.

MARTIN A. MILLER
Duke University

T. M. KITANINA. *Khlebnaia torgovlia Rossii v 1875–1914 gg. (Ocherki pravitel'stvennoi politiki)* [Russian Grain Trade, 1875–1914 (Essays on Government Policy)]. Edited by S. I. POTOLOV. Leningrad: Izdatel'stvo "Nauka," Leningradskoe otdelenie. 1978. Pp. 286.

In this admirably researched book T. M. Kitanina explores two general issues: (1) the organization of the Russian grain trade from 1875 to 1914; and (2) the evolution of government policy on the grain trade during the same period. Her discussion of the first issue is an unoriginal, but thorough, treatment of the transition from the weakly developed, decentralized rural commercial system that prevailed in 1875 to the organized rural capitalism of the early twentieth century. Kitanina demonstrates how the small rural merchantry, once described as "that hundred-thousand man army of grain traders" (p. 163), was displaced by large commercial firms located in the capital and port cities and by the government itself. The process of displacement began slowly in the great depression of 1875–96 but was accelerated in the late 1890s when the central government committed vast resources to the building of a national grain elevator system and commercial railroads. By 1910 Russian rural commerce was controlled mostly by concerns like I. Stakheev and Company, a firm that had close ties to major banks and that took a direct hand in every facet of trade from finance to storage to shipping.

Kitanina's analysis of government policy on the grain trade is the heart of her book. Drawing mainly on unexploited archival resources, Kitanina explains government policy on three questions: railroad duties on grain shipments; grain procurement by the army; and customs charges by Russia and foreign nations that affected Russian agriculture. Her conclusions are that the government consistently played a high-profile "interventionist" role in rural commerce and that government policy was designed as much to subsidize the landed gentry as to promote the development of capitalism in Russia. The most controversial point is Kitanina's description of Finance Minister Sergei Witte as a qualified supporter of the landed gentry. For example, she sees Witte's adjustments of the 1889 railroad duty legislation as attempts to assist the gentry of central Russia and to balance gentry interests against those of "bourgeois" Siberian grain producers. Witte was not the convinced advocate of compulsory grain exports sometimes depicted in historical literature. In fact, his policies were much more complicated than other historians have realized. Kitanina styles Witte as a complex, even contradictory, conservative, an adherent of economic "counter-reforms."

Kitanina's revisionist portrait of Witte is interesting, but finally unconvincing. Witte consistently resisted the Ministry of Internal Affairs' policy of support for the nobility, and his main complaint against V. K. Plehve was that the latter wanted to govern "in the spirit of the nobility." Witte's compromises on the railroad duties question should be read as tactical concessions in the face of enormous pressure from the gentry within and without the government, not as part of a master plan to subsidize a dying class. Yet even if one disagrees with

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Kitanina's interpretation of the evidence, one is deeply indebted to her for this solid book.

G. M. HAMBURG
Cornell College

ROMUALD WOJNA. *Walka o ziemię w Rosji w 1917 roku* [The Struggle for Land in Russia in 1917]. Wrocław: Ossolineum. 1977. Pp. 290. 65 Zł.

This work by the Polish historian Romuald Wojna, who specializes in Soviet Russian agrarian history, presents a real challenge to the Western reviewer for two reasons. For one, it offers much unknown material, data, details, and insights about events in the Russian countryside between the February and October Revolutions. Secondly, it exemplifies the present state of official Polish historiography, which, in contrast to Soviet historiography, is attempting to establish more balanced analyses and conclusions. It is Marxist but not narrowly dogmatic, open to empirical testing but unwilling to admit the limits of Marxian historical dialectic. A case in point is that Wojna feels free to critically discuss Lenin's misinterpretations and to point to his inconsistencies, and yet most of the time he lets him have the "last word" in the pronouncements of the "final truth." This partial deviation from absolute righteousness makes it more interesting to read this methodologically well-organized study.

The author's expressed task was to identify the degree and scope of the agrarian impact upon the crucial events between the two revolutions of 1917. This required investigation of the causes that stimulated peasant action, description and analysis of the programs and attitudes of the various political parties and social groups toward the peasant question, and the identification of interaction and responses between the peasantry and the respective parties.

To this end, Wojna developed his theme in seven chapters: the agrarian question prior to 1917; agrarian programs of the existing political parties from Bolsheviks to the Union of Landowners; the February Revolution and the village; development of village organizations under the influence of the moderates—April and May; the period of relative stability—June; between coups of the left and right; before the seizure of power by the left and the fall of the moderates—September and October.

The growing restlessness in the village and its contribution to further collapse and disintegration of the system is illustrated by statistics. Between February and October, 5,416 acts of violence were registered in ethnic Russia and 8,195 for the whole of Russia. Of the actions, 80 percent were directed against large-estate owners (*pomeshchiki*) and only 20 percent against wealthy peasants (*kulaks*), which underlines the social character of the movement and casts doubt on the existence of a "class war in villages" as Lenin wanted to have it. In fact, "class war" was forced upon the peasantry by the Soviet regime after Lenin's seizure of power. The majority of Russian and even more non-Russian peasants gravitated toward the capitalistic form of economy and not toward socialism (p. 247). Moreover, Wojna suggests that the Provisional Government failed precisely because of its inability to grasp and to resolve the agrarian issue to the satisfaction of the peasantry; hence, Lenin's propaganda about the common front of the workers with the peasantry remains an exaggerated claim, for in 1917 "the key to the situation in Russia was agrarian reform and the need to bring war to an end" (p. 252).

In the final analysis, it was not the Bolsheviks' influence, but the failure of the liberals, moderates, and conservatives to agree and to act on the agrarian question that determined the flow of events and the history of Russia as well. This conclusion indeed differs from the Soviet version.

This well-documented study also offers an impressive list of sources and monographs, mainly Soviet, but also several Western works, in addition to the name, geographical, and subject index. Aside from some stereotyped generalizations, the study deserves attention and a place on the expert's shelf.

STEPHAN M. HORAK
Eastern Illinois University

EVAN MAWDSLEY. *The Russian Revolution and the Baltic Fleet: War and Politics, February 1917–April 1918.* (Studies in Russian and East European History.) New York: Barnes and Noble. 1978. Pp. xiv, 213. $23.50.

For many years writers on the Revolutions of 1917 concentrated on the leaders and ideologies contesting for Russian power. The view was essentially from the top, with only a very general examination of the contemporary conditions in the country and among the various elements of the population. Now several Western scholars have focused their research on the workers, soldiers, Red Guards, peasants, and sailors. Perhaps because the silhouette of a cruiser is one of the most persistent images of the Bolshevik seizure of power, the navy has received special attention.

Evan Mawdsley's study of the revolution in the Baltic Fleet appeared a few months after the reviewer's *Sailors in Revolt: The Russian Baltic Fleet in 1917*. With the exception of a more extensive treatment of the pre-1917 background in the latter, the subject parameters and emphases are similar: the concentration is on the fleet in revolution through April 1918. Mawdsley provides some different or

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additional details on key episodes and personalities. Together, or singly, the books document the quantity of sources on the subject, ranging from the observations of a variety of contemporaries, the many productions of Soviet scholars, and materials in Finnish and other archives outside the Soviet Union.

In describing the fascinating drama of discord and violence in the February/March revolution in the fleet, Mawdsley fails to detect splits and divisions among the officers that hampered their ability to respond effectively to the rapid breakdown of traditional order and discipline. The dynamics and psychology of both revolutionary and naval actions, though admittedly difficult to reconstruct, are slighted by Mawdsley. He attributes the surprising ability of the Baltic Fleet to fight at Moon Sound in early October to the ships' inactivity, out of range of danger, thus underplaying the sorties made during the war, the real threat of mines (both Russian and German), and the imagined danger from sabotage. Esprit de corps becomes inconsequential: "The will of individuals was irrelevant, because a warship carried all its crew together into battle" (p. 94).

The background to the seizure of power in October/November is treated with sophistication and balance. But after developing a considerable case for the importance of the sailors in those vital days and describing the many steps being taken to mobilize them for action, Mawdsley supports Robert Daniels's interpretation that the Bolsheviks were acting defensively until Lenin's crucial midnight arrival at the Smolny Institute. He then bravely tackles the question (while noting its difficulty) of whether the sailors were decisive in launching Soviet power: "I think however that the answer is no, if only because the pro-Kerensky forces were so feeble: the sailors would have been more necessary had resistance been greater" (p. 115). Surely the Baltic sailors had contributed during 1917 to the situation of the government in those October days.

Mawdley's book provides a balanced examination of an important institution vital to a better understanding of Russia in 1917 and 1918. It can also be read with the reviewer's as an exercise in how two historians, working independently on the same subject at about the same time, come to similar general conclusions but with varying nuances of style, detail, and interpretation.

NORMAN E. SAUL
University of Kansas

P. M. CHIRKOV. *Reshenie zhenskogo voprosa v SSSR (1917–1937 g.g.)* [The Solution of the Woman Question in the USSR (1917–37)]. Moscow: Izdatel'stvo "Mysl'." 1978. Pp. 254. 90 k.

Soviet historians do not devote much energy to the history of women in the Soviet period. After reading this book, one can only conclude that it is just as well. Clearly a response to the women's movement in the West, the book purports to refute the ideology of Western "bourgeois feminists" by filling the last remaining gap in Soviet historiography, that is, to discuss the history of the special apparatuses for work among women in party, state, and social organs. There is actually some promise in the stated intent: Western scholarship has recently produced some excellent general works on women in the Soviet Union, but the field is in its infancy. A study of the distribution of work with women among various party organs after the demise of the *Zhenotdel* (Women's Section of the Party Secretariat) in 1930 could be valuable indeed. What emerges, however, is, first, a diatribe against Western feminism that is straight out of the late nineteenth and early twentieth centuries. The defense of the Marxist formulation of the "woman question" is couched in terms that were interesting and valid in the context of nineteenth- and early twentieth-century reality. But the fact that the focus of contemporary feminism is no longer the acquisition of the vote seems to have escaped the author. It is simply silly these days to claim that "many bourgeois idealogues claim that socialism destroys the family" (p. 214) or to state as a matter of fact that women in the capitalist countries today work mainly in the service sector and as domestic servants.

Second, the greater part of the book is a formalistic narrative of resolutions taken at party congresses. After each set of resolutions the author states either that they were fulfilled or that they were not fulfilled. There is no discussion of the proceedings, the debates, the conflicts that accompanied these resolutions, nor, if they were implemented, how the implementation occurred in real life. The rare reference to how the objects of the resolutions—real women—responded to the resolutions is always followed by a totally contradictory and highly idealized conclusion. For example, the author reveals that in 1930 it was often difficult to attract girls into industry because they still considered marriage their primary goal. Two pages later we are told that women flocked to socially productive labor (that is, factory work) because of their ideological convictions: "the new socialist morality gave birth to a need to serve their socialist motherland" (p. 125). The missing link between these two assertions—not to speak of the missing documentation—need not be labored.

The book is valuable as a gauge of official Soviet attitudes toward women. The footnotes may be a useful guide to archival holdings for potential Western scholars. Beyond that, there is little to be said for it unless one accepts its conclusion that the

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woman question, a product of capitalism, was resolved in the Soviet period of socialist construction.

ROSE GLICKMAN
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GEORGE M. ENTEEN. *The Soviet Scholar-Bureaucrat: M. N. Pokrovskii and the Society of Marxist Historians.* University Park: Pennsylvania State University Press. 1978. Pp. xii, 236. $15.00.

George M. Enteen has written a sympathetic and compelling study, the first in any Western language, of the founder of Soviet Marxist historical scholarship. The focus is on the last few years of M. N. Pokrovskii's career (1927–32), when he was battling on two fronts: against the relics of "bourgeois" (that is, non-Marxist) ideology and also against the intrigues of Stalin and his followers. Like many middle-class converts to Bolshevism, Pokrovskii had embraced the new creed with apostolic fervor without totally forsaking the humane ideals of the old Russian intelligentsia. During the NEP era, in accord with its relatively tolerant atmosphere, he had sought to persuade non-Marxist scholars to cooperate with the regime on a mutually profitable basis. Simultaneously, he had developed a highly partisan, and somewhat inconsistent, Marxist interpretation of Russian history that became standard doctrine throughout the Soviet educational establishment. This was based on the assumption that class conflict had led to the same social transformations in Russia as in Western Europe and that the Bolshevik overturn was therefore not a chance event, explicable in terms of the particularities of Russia's experience, but rather the predestined harbinger of an international proletarian revolution. This view was clearly better attuned to the age of Lenin and Trotsky than it was to the late 1920s, when Stalin's supremacy was bringing about a far-reaching ideological reorientation. A bitter struggle ensued between rival historical schemas and theorems in which Pokrovskii was obliged to acknowledge his errors; and after his death his reputation suffered a total eclipse.

The irony of his situation was that he himself had eased his persecutors' task by insisting that Marxist teaching alone was truly scientific and therefore worthy of dissemination in a "proletarian dictatorship." This point is not lost upon Enteen, whose detailed account of these debates and maneuvers carries conviction. He is right to point out that genuine discussion was still possible as late as 1930–31, although the intellectual climate was largely shaped by the cynical manipulation of ideas characteristic of modern totalitarianism (a term eschewed here). Regrettably, Enteen does not offer an independent assessment of the historical validity of the positions taken on particular issues (for example, "mercantile capitalism") or indeed make it sufficiently clear where he stands on broader questions of historical interpretation: he evidently has reservations about the Western liberal as well as the Soviet Marxist approach. This fuzziness over fundamentals weakens the force of his arguments; nevertheless, he has analyzed with great facility an abundance of published source material, much of it very rare, and has also taken the trouble to interview some surviving Soviet historians who participated in the debates of the 1920s. This is a useful contribution to the growing body of literature on the impact of Stalinism on Soviet intellectual life.

JOHN KEEP
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A. A. SHEVIAKOV. *Sovetsko-rumynskie otnosheniia i problema evropeiskoi bezopasnosti, 1932–1939* [Soviet-Rumanian Relations and the Problem of European Security, 1932–39]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 381. 2 r. 82 k.

A. A. Sheviakov ably guides the initiated reader through a maze of interwar diplomatic doings. His thesis is that the Soviet Union consistently sought peace during the 1930s; however, imperialistic powers—both democratic and fascist—including Rumania, foiled Soviet efforts. Sheviakov endeavors to correct bourgeois historical interpretations that distort the peaceful orientation of Soviet foreign policy and, further, that justify the aggressive or appeasing designs of Rumania and some West European states. He blames the Rumanian government for not cooperating with the Soviet Union's pacific mission and for pursuing, instead, an anti-Soviet policy.

Sheviakov describes Rumanian-Soviet relations in detail. On the one hand, he tells about Rumanian-Soviet concurrence in defining aggression in the London Convention of 1933, in establishing diplomatic relations between the two states in 1934, and in negotiating for a mutual aid pact. On the other hand, Rumanian statesmen refused to sign a nonaggression treaty with the Russians—despite the precedent of Soviet-French and Soviet-Polish accords; moreover, negotiations for the Soviet-Rumanian mutual aid pact eventually foundered in 1936. As a result, Rumanian leaders abandoned the idea of collective security and—together with Germany, Italy, and Poland—adopted an anti-Soviet course.

The key to Rumanian foreign policy decisions was, according to Sheviakov, Bessarabia. Rumanian statesmen sought to preserve the unified Rumanian state that had been created after World

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War I and even, perchance, to enlarge their country at the Soviet Union's expense. In the early 1930s the Rumanians' decision to postpone a settlement of the Bessarabian question facilitated the opening of diplomatic relations between the two countries; later, however, the Rumanian demand that the Soviet Union formally recognize the cession of Bessarabia to Rumania torpedoed negotiations for mutual aid. Sheviakov thinks that Rumanian as well as English, French, and Polish statesmen erred in assuming that, owing to ideological considerations, Germany would attack the Soviet Union but would not threaten other states. The failure of collective security as well as Rumanian cooperation with the Axis powers provided opportunities for Germany to remilitarize the Rhineland, for Italy to attack Ethiopia, for Germany and Italy to intervene in Spain, and for Germany to invade Austria and Czechoslovakia. These happenings, says Sheviakov, were to imperil Rumania's actual existence as well as the cause of peace in Europe.

This book's importance lies more in Sheviakov's fresh archival evidence than in either his outline of events or his predictable thesis. Valuable chapters covering the years 1934 to 1938 contain Sheviakov's findings from unpublished manuscript materials in the Soviet Union's ministry of foreign trade archives, the Rumanian state archives, and the Rumanian Communist Party archives. In addition, the author explores published documentary collections, contemporary newspapers, and memoirs. Surprisingly, however, Sheviakov ignores pertinent memoirs by the Rumanian diplomat Mihail Sturdza and the Rumanian historian Nicolae Iorga (specifically, volume seven covering 1932–38). Also missing from his otherwise helpful bibliography are germane dissertations by Soviet scholars such as Iov Vasil'evich Turku and Aleksandr Ivanovich Sizonenko.

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I. V. MIKHUTINA. *Sovetsko-pol'skie otnosheniia, 1931–1935 gg.* [Soviet-Polish Relations, 1931–35]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 285. 1 r. 20 k.

During the years 1931–35 relations between the USSR and Poland were dominated by four major issues: the signing of the Soviet-Polish pact of nonaggression, the declaration of nonaggression between Poland and Germany, and the questions of collective security and the Eastern pact. These topics are covered in four well-balanced chapters.

According to I. V. Mikhutina, Poland—faced with the consequences of the Locarno Treaty and its bad relations with Germany—initiated its new Eastern policy in 1926 to achieve better relations with the Soviet Union. After prolonged negotiations the two countries signed a pact of nonaggression in 1932 (pp. 19–20). But the author argues that Poland considered the pact only as a means to establish its independence from the Western powers and to improve its relations with Germany (p. 63). Mikhutina considers the Polish policy of equilibrium—the maintenance of balance between Germany and the USSR—as illusory (pp. 181–82). Instead of following up the 1932 agreement with a pact of mutual assistance with the Soviet Union, Poland—to isolate the USSR—initiated its imperialistic policy of attempting to organize Eastern Europe into a bloc under its own control (p. 279). Poland was already following a pro-German and anti-Soviet policy (p. 6), which was guided by class interests and traditional anti-sovietism (p. 260). Thus, the Polish-German declaration of nonaggression of 1934 is viewed here as an attempt to neutralize the anti-Polish aspects of the Locarno Treaty (p. 149) and to prevent the formation of an anti-German coalition, which would include the Soviet Union (p. 278). The Polish-German declaration, Mikhutina argues, was basically anti-Soviet in character. The Polish policy of equilibrium was used first to cover up this fact and then as an excuse to turn down the Eastern pact (pp. 278–79, 184–85). But, according to the author, the realistic alternative was a closer Polish-Soviet cooperation that would have strengthened Soviet attempts at collective security through Polish participation in the Franco-Soviet understanding (p. 278).

The book contains a selected bibliography, which includes some major Western sources; but, unfortunately, there is no index. Western readers will wonder why the author cites Lenin's writings ten times. Furthermore, the contributions of Western historians to Soviet-Polish relations are arbitrarily dismissed as "bourgeois falsifications" presented "from the position of antisovietism and anticommunism" (p. 9). The author is familiar with the contributions of Polish historians, but Mikhutina uses their findings selectively. Generally, the book is based mainly on Soviet and foreign published documents, Polish archival material, and Polish and Soviet press reports. But no Soviet archival diplomatic sources were used. Because of this alone, few important questions of Soviet policies are satisfactorily explained, and the reader will search in vain for a clear and precise presentation of Soviet motives, aims, interests, and policies. Consequently, nothing new is added to our knowledge of the subject. Specialists would have been grateful for additional information based on Soviet sources on such unresolved questions as the Miedziński-Radek negotiations in 1933. Instead of giving us a clear explanation of Soviet objectives in Radek's proposals, all we get is a summary (pp. 95–96, 116–21) of Miedziński's articles published in 1963 in the Paris

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Kultura. Interestingly enough, in this summary Radek is not even identified by name! Nevertheless, Mikhutina's book should be considered as a positive addition to Soviet historiography—it introduces the Soviet reader to some Western and Polish sources. But Western specialists will find here little that is new to them.

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WILLIAM O. MCCAGG, JR. *Stalin Embattled, 1943–1948*. Detroit: Wayne State University Press. 1978. Pp. 423. $18.95.

William O. McCagg, Jr., offers an interesting thesis: Western policymakers and historians have generally held a false view of the motives underlying Stalin's foreign policy following World War II. They believed that he concealed his revolutionary plans from his wartime allies then openly pursued a variety of revolutionary objectives after the war. But, according to McCagg, Stalin really was the man of peace they had earlier thought him to be. Soviet postwar foreign policy was turned from its course by Stalin's temporary loss of control: "At the end of the war . . . his regime broke step with his statism, and the Communist Party briefly resumed its leading role within the Soviet system, encouraging the Red Army and the Communist parties abroad to press for revolutionary power. Their efforts created the illusion during 1948 that Stalin stood for world-wide revolution . . ." (p. 14). The author portrays Stalin as a man who early had "accepted rigid isolationism as the best of foreign policies" (p. 36) and whose postwar policy was distorted by difficulties in controlling a revived Communist Party internally and revolutionary Communist Parties abroad. He thus directs his attention to two main areas: the relationship with foreign Communist leaders and the linkages between Soviet domestic and foreign policy.

Perhaps the most useful part of the book is the discussion of Stalin's relationship with the foreign party leaders, especially those from the fascist-occupied countries of Europe. It includes much valuable information on the attitudes and problems of those leaders and their conflicts with Stalin, adding considerable evidence to support the view that Stalin was deeply distrustful of individuals and parties he could not control. To sustain the main thesis, however, would require demonstrating not just Stalin's compulsion to control foreign parties, but his outright rejection of their revolutionary mission (as opposed to insistence on close coordination of revolutionary efforts with Soviet diplomatic interests). This is not done, nor is it made clear that Stalin's behavior is significantly different from that of Comintern strategists of the early 1920s. Moreover, in the context of the author's thesis one would expect some evidence of ties between foreign Communist leaders and "genuine" revolutionaries within the Soviet party who disagreed with Stalin's policy.

The book does not demonstrate that Stalin's encouragement of revolutionism in 1948 resulted from an intraparty challenge to his power. McCagg writes that Stalin "may not have been a prisoner of the Politburo, but he was surely the prisoner of the no longer total control system over which he presided" (p. 23). But the precise nature of this "no longer complete control" is unclear, as is the evidence for its reduction. Similarly, it is difficult to relate the discussion of domestic policy to that of foreign policy. The author promises to provide insight into "the problem of Stalin's intentions by constructing a framework for assessing what he 'wanted' in the areas of agriculture and culture, and then (by process of analogy) in international affairs" (p. 243). The analysis of agricultural and cultural policy is sound, but the connection with foreign policy and the larger thesis is less clear, except in the broad sense that Stalin had severe internal problems that recommended a cautious foreign policy.

In his conclusion the author writes that Stalin's achievement of postwar "stability inside the Soviet Union and controls on the Communists abroad" were a vital contribution to peace and that "he wanted peace" (p. 312), a fact unappreciated by Western statesmen. Doubtless Stalin did want peace, but it is doubtful that he "strove . . . hard throughout his career to escape revolution" (p. 311). Trotsky offered such a view (criticizing Stalin's China policy in the twenties), and similar opinions have had wide currency in the literature of Soviet studies since. The evidence used to support such a view can be used equally cogently to argue that Stalin did well with the difficult task of blending Soviet state interests with the interests of Communist revolution. But whether accepting McCagg's conclusions on this and other issues or not, one is bound to enjoy an erudite and vigorous challenge to the accepted wisdom about Stalin's purposes and policies in a vital period.

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NEAR EAST

AMNON COHEN and BERNARD LEWIS. *Population and Revenue in the Towns of Palestine in the Sixteenth Century*. Princeton: Princeton University Press. 1978. Pp. xii, 199, 15. $17.50.

This work is a study of the *taḥrīr* registers prepared by the Ottoman administration in Palestine in the sixteenth century. The registers, which contain a

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survey of population and revenues, were prepared for the guidance of tax collecting authorities upon the conquest of a new province, the beginning of a new reign, or whenever political, economic, or demographic events seemed to require revisions. In Palestine, which was absorbed into the Ottoman Empire in 1517, six surveys were taken in the course of the sixteenth century—in 1525–26, 1538–39, 1548–49, 1553–57, 1562–67, 1596–97. This book gives a full translation and study of the surveys taken in the towns of Palestine—Jerusalem, Hebron, Gaza, Ramla, Nabulus, and Safed—in this period. The inhabitants are enumerated by town quarter and fiscal subcategory. Taxes on agricultural lands, Bedouin pastures, trade, and industry are detailed.

From these registers the authors draw a number of conclusions. The towns of Palestine were very modest in size, for none had a population of more than six thousand. Population grew dramatically between 1521 and 1548 due to increased security and prosperity but then declined in the later part of the century in circumstances of declining trade, depreciation of currency, political oppression, and nomad banditry. By the end of the century, the total population was still one-third higher than at the beginning. The authors note that Hebron increased in population and economic importance. Toward the middle of the century it was reclassified from village to town status. Safed was the most important cloth-producing center, with Nabulus second. The authors conclude from their examination of the data on quarters that, although there was some tendency for religious, ethnic, and Bedouin minorities to live together, town quarters tended to have heterogeneous populations. Increasingly effective central administration reduced the importance of the Shaykh al-Ḥāra as the local official responsible to the authorities for good order and the collection of taxes.

On the whole, this book is an exceedingly competent translation and analysis of the Ottoman *taḥrīr* registers, but, being limited to these documents and the information which can be garnered from them, it remains a very specialized presentation. The authors work entirely within the registers and do not use other sources except to clarify the textual meaning. The conclusions are narrow and give the unfortunate impression that Palestine in the sixteenth century holds little interest for the historian or that *taḥrīr* registers are, for all the technical expertise it takes to study them, rather unyielding. In fact, neither is the case. These archival sources (a source scarcely to be found in the Middle East before the Ottoman period) are novel, give us detailed information beyond the dreams of historians confined to literary materials, and present fascinating technical problems. Because of all this, it is necessary to integrate them with other materials to bring the history of Palestine in the sixteenth century to life and to establish the place of the province in the larger Ottoman context and the significance of this period in the ongoing history of the region.

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BERNARD WASSERSTEIN. *The British in Palestine: The Mandatory Government and the Arab-Jewish Conflict, 1917–1929.* (Royal Historical Society Studies in History Series, number 10.) London: The Society. 1978. Pp. xii, 278. £8.25.

Recent scholarship on twentieth-century Palestine has focused upon the roots of Palestinian nationalism and its relationship with Zionism and the Jews both before World War I (in the work of Neville Mandel) and in the early mandate years (that of Yehoshua Porath), supplementing existing works on the evolution of the Jewish community during the mandate. Long missing, however, has been a thorough scholarly study of the third, British component in the mandate triangle. Bernard Wasserstein's study—originally an Oxford thesis—goes some way to fill this gap. He offers no detailed study of policy formation in London, but he does discuss the Anglo-Jewish conflict in the framework of the Palestinian administration's struggle to find a safe path between the needs of civil peace and the Balfour Declaration's obligation to facilitate the establishment of a Jewish national home.

Although *The British in Palestine* considers specific policies and their local application, Wasserstein works mainly through the individuals concerned. Since his evidence is necessarily drawn mainly from British and Israeli archives, his portraits of British and Jewish elements in the administration's service are more comprehensive than those of Arabs. Sketches of British gentiles (Clayton, Deeds, Meinertzhagen, Storrs), British Jews (Bentwich, Hyamson, Nurock, Samuel), and a few Arabs (Antonius, 'Aref al-'Aref, Musa al-'Alami, Ruhi Bey 'Abd al-Hadi) are both interesting and educational. Wasserstein writes economically and well, and even the most advanced scholar will find valuable information here.

Nothing was simple in the mandate; every issue had to be considered in the light of its religious and national implications—and every administrator on the basis of his personal relationship to those considerations. This bureaucrat favored one side, that another; it was not just Arab versus Jew with Britain holding the ring. That Zionist and non-Zionist Jews could disagree is a commonplace; less familiar are the struggles between Christian Arab and Muslim Arab or among family-based Muslim factions. The

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temptation to use money or power to influence affairs was irresistible; thus Zionist funds augmented the low salaries of "friendly" Arabs and persuaded the controller of ports to ignore smuggled *Haganah* arms. Wasserstein documents such matters with diligence and care, above all in relations between Jewish and British leaders. His conclusion seems sustained: "Predominant opinion among British officials in Palestine between 1917 and 1929 was that the policy of the Balfour Declaration was a dangerous and costly mistake" (p. 157).

Why this was so needs more discussion, however. For example, Wasserstein has missed an opportunity to test the common assertion that Palestine was a British refuge for second-class timeservers, happy only when governing quiescent rural Arabs. Although the reader is told how many of the 3,400 Palestinian officials in 1929 (when the study ends) were British or Palestinian, Muslim or Jew, at senior and junior levels, Wasserstein provides no information on recruitment, education, social background, and the like. Overall, however, Wasserstein's book is useful and informative and should find a place in every library aspiring to Near East comprehension.

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SIMON SCHAMA. *Two Rothschilds and the Land of Israel.* New York: Alfred A. Knopf. 1978. Pp. 399. $15.95.

The two Rothschilds referred to in the title of this book are Baron Edmond (1845–1934) and his son James (1878–1957), both of whom devoted much of their lives and money to the furtherance of Jewish "colonization," that is, settlement, in Palestine. Simon Schama presents a broadly based account of these two Rothschilds' relations with the *Yishuv* (the Jewish community of pre-Israel Palestine), although, of necessity, it is Edmond whose work and personality dominate the book. It was he who, as early as 1883, began to give his support to the first Jewish settlements in Palestine, which had been founded only shortly before; and he continued to do so, through the Jewish Colonization Association (ICA), and later the Palestine Jewish Colonization Association (PICA), until his death more than fifty years later. The prodigious enterprise undertaken by Baron Edmond and the stresses and tribulations to which he became exposed as a result of his lifelong devotion to this cause are the central subject of the book under review.

Perhaps the most interesting point is that within eighteen years after his conflict with Herzl (1896), whose political Zionism Edmond opposed (and to whom the author has a pronounced, and puzzling, antipathy), Edmond himself had advanced so far along the road to political Zionism that Chaim Weizmann was taken aback by discovering this (p. 192). In fact, Edmond even revived the old Herzlian idea of a chartered company for the financing and development of Jewish Palestine, and it was he who urged Weizmann to return to England and to sound out political opinion on the highest level (p. 195).

James, although born in Paris, became at an early age a British subject and in 1924 president of PICA, in which capacity he carried on the work begun by his father. Most important was his unflinching advocacy of the Jewish cause in Parliament, of which he became a Liberal member in 1929. He, too, had an unflagging interest in the *Yishuv* and later in Israel and was a staunch and generous supporter of the young Jewish state.

Schama's book invites criticism on several counts. It is overloaded with minor technical details (such as how much wine Turkey exported in 1890, or how much wine this or that "colony" produced in certain years), while leaving such major issues as the sources of Baron Edmond's devotion to Palestine undiscussed. It is replete with faulty transliterations and translations of Hebrew, Arabic, and other names and terms. For example, Chief Rabbi Hertz's name is spelled "Heraz"; "Nadiv" does not mean "successor organization to PICA"; "Avoda Ivrit" means neither "Jewish laborer" nor the "conquest of labor" as it is variously defined; "hayal" is not "Turkish militia"; "Bikkur holim" is not "sanctuary for the sick" but "visiting the sick"; "Shomron" is not "the Samaritan coastal area"; "partition of hitherto common land" is not "mafruz" but "ifraz." Schama's figures are occasionally suspect, too—how could there have been 3,000 tailors among the total of 15,000 Jews in Jerusalem in 1882 (pp. 58, 346)?

Such errors, much too numerous, regrettably detract from the value of this otherwise important study.

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NEIL CAPLAN. *Palestine Jewry and the Arab Question, 1917–1925.* Totowa, N.J.: Frank Cass. 1978. Pp. xvi, 268. $19.50.

M. MOSSEK. *Palestine Immigration Policy under Sir Herbert Samuel: British, Zionist and Arab Attitudes.* Totowa, N.J.: Frank Cass. 1978. Pp. xii, 179. $19.50.

In the past decade the historiography of modern Palestine, hitherto a desert of tired apologetics and repetitive propaganda, has been refreshed by a steady flow of original monographs by a young generation of scholars. The products of the labor in the Ottoman, British, and Israeli archives of historians

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such as Amnon Cohen, Moshe Ma'oz, Neville Mandel, and Yehoshua Porath have created a new framework for thinking about the history of Palestine and have set high standards of objectivity and sophistication. To those studies are now added two important books, both major contributions to scholarship by former pupils of Elie Kedourie at London University. Both M. Mossek and Neil Caplan employ hitherto unexplored archival sources to excellent effect, and both, while focusing attention on apparently narrow areas, come to original conclusions that shed light on problems of broad historical concern.

Mossek's study of Colonial Office and Palestine Government papers enables him to modify considerably the received view of the relationship between the British mandatory authorities and the Zionist Organization on the key issue of Jewish immigration. Concentrating on the formative period of office of the first high commissioner, Sir Herbert Samuel (the only Jew to hold the office), Mossek shows that in this period there was complete agreement between the Zionist leaders and the high commissioner in objecting to an "open door" policy of free immigration. Indeed, the Zionists, beset by acute financial embarrassments, incurred complaint from Samuel in late 1920, because they were seeking to reduce the immigration schedule from 16,500 families (the figure set by the government) to a mere thousand. Mossek argues that the principle of "economic absorptive capacity," adopted as the criterion for establishing permitted immigration levels, was a response by the government of Palestine less to the pressure of Arab rioters in May 1921 than to the near bankruptcy of the Zionist Organization and its inability to absorb massive numbers of immigrants with the meager resources then at its disposal. The real difference between the Zionists and the British in this period, concludes Mossek, was over the question of who was to do the selecting and regulating of immigrants, not, as is often assumed (thinking backwards from the 1940s), over the issue of numbers.

The distorting mirror of hindsight has deformed discussion of other aspects of Palestine's history in this period. How commonplace has become the notion that the early Zionists believed that Palestine was "empty" and that they failed to give serious consideration to the potential opposition to their scheme of the indigenous Arab population. Neville Mandel, in his masterly book *The Arabs and Zionism before World War I*, recently showed how manifest this opposition had become before 1914 and also how seriously it was considered by many leading Zionists. Caplan presents a close-up view of the different trends of thought on the "Arab question" among the *Yishuv* (the Jewish population of Palestine) during the formative early years of the British presence in Palestine. He identifies two basically different approaches to the problem. An "optimistic" minority maintained that it was possible to resolve the apparent conflict between Zionist and Arab interests in Palestine. This was the stream of thinking that later flowed into the binationalist groups such as *Brith Shalom*. The "pessimistic" majority among the *Yishuv*, on the other hand, dissented from the proposition that it was possible to harmonize interests that clashed so fundamentally. As early as 1919 David Ben-Gurion gave expression to the latter view when he declared to the *Va'ad Zmani* (the Provisional Council of the *Yishuv*): "There is no solution to this question. No solution! There is a gulf, and nothing can fill this gulf. It is possible to resolve the conflict between Jewish and Arab interests [only] by sophistry. . . . We must recognize this situation." The pessimists did not, of course, draw the conclusion that the entire Zionist enterprise should be abandoned. Rather they argued that it was a futile form of propagandistic self-delusion to seek to persuade the Palestinian Arabs that it was in their long-term political interest to come to terms with Zionism. The answer instead was to realize the practical aims of Zionism as quickly as possible, and then, having by immigration built a Jewish majority, having established a self-contained Jewish economy, and having trained and armed a Jewish defense force (the *Haganah*), to present the Arabs with a fait accompli. Caplan analyzes the nuances and intricacies of the internal debate among the *Yishuv* with a remarkable sensitivity to the atmosphere of the period. His discussion of the thinking behind the so-called silver bridge to Arab-Jewish understanding (that is, bribery, a topic generally glossed over by Zionist historians, particularly those of the left) and of Zionist attitudes to the British authorities is both lucid and original. Caplan concludes that the *Yishuv*, in this period, underwent a "gradual learning experience." Students of modern Middle Eastern history will have similar feelings upon reading these two valuable books.

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AFRICA

J. S. BIRKS *Across the Savannas to Mecca: The Overland Pilgrimage Route from West Africa.* London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1978. Pp. xiii, 161. $20.00.

J. S. Birks's short book analyzes the Muslim pilgrimage (*hajj*) and West African communities in the contemporary Sudan, focusing on the western province of Darfur. Pilgrimage across the savannas from West Africa to Arabia began in the seventeenth

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century, but only after 1750 did this network rival trans-Saharan itineraries. By this time, Muslim states controlled most of the savanna, making travel more secure, and Islamic reform movements in West Africa had expanded the numbers of Muslim faithful, many of whom were poor and unable to afford the more costly Saharan-Mediterranean voyage. Most pilgrims came from Hausaland and Borno, and because the journey was long and arduous they often halted along the way. Many settled in the Sudan because conditions were more secure and because the area was close enough to Arabia to serve as a staging point for the final leg of the journey. As a result, the Sudan shelters a sizable West African minority.

Birks introduces and concludes his study by looking at pilgrimage traditions in other religions, together with Islamic variations. He then broadens the discussion to suggest that such observances are an overlooked form of population mobility. In between, the book's focus narrows progressively in time and space. The second chapter traces the history of the West African *hajj* to 1900, while the next is devoted to the twentieth century. The remaining sections deal primarily with the contemporary Sudan, although pilgrim biographies and histories of West African settlements add a diachronic dimension.

Some of Birks's conclusions support those of earlier observers. Just as routes shifted away from insecure or inhospitable regions in the precolonial period, so contemporary travelers avoid hostile authorities, particularly along the Chad-Sudan border. Birks also notes that the *hajj* instills a special sense of purpose in the West African communities remaining on the routes, which inhibits incorporation into Sudanese society. And his surveys of pilgrim activities indicate that the precolonial pattern of stopping to work continues; as before, people halt to farm and teach, but now they may also work in the Gezira cotton scheme or seek employment as wage laborers.

But Birks also offers new perspectives. Contary to common expectation, most pilgrims travel with family members. Cost comparisons are also surprising. In 1971 a charter air ticket from Kano to Mecca cost S£110 (Sudanese) while the overland trip required S£125. (The advantage of the overland trek is that a large lump-sum payment is seldom necessary.) Finally, the overland *hajj* takes an average of nine years today, two years more than in the 1940s, largely because African states have not welcomed pilgrims.

Across the Savannas to Mecca is well illustrated with forty-nine maps, figures, and tables. The book is short, but unfortunately the narrative is not always correspondingly concise. And the dichotomy drawn between Hausa pilgrims and Fulani herders may be overemphasized. Further west in Bagirmi, the history of these peoples has long been intertwined, as indeed it has been in Hausaland, leading one to suspect that even if such is not the case among West Africans in the Sudan today, it may well have been so historically.

DENNIS D. CORDELL
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HELMUTH STOECKER *et al. Drang Nach Afrika: Die koloniale Expansionspolitik und Herrschaft des deutschen Imperialismus in Afrika von den Anfängen bis zum Ende des zweiten Weltkrieges.* Berlin: Akademie Verlag. 1977. Pp. 370. 38 M.

East German scholars concerned with the history of German imperialism in Africa and the experience of countries once governed by Germany have been drawn together in this collective volume. It is parallel in many ways to *Britain and Germany in Africa* (1967), the festschrift for Harry Rudin published a decade earlier. As the editor of the East German collection would quickly point out, however, the books are products of two different traditions, "bourgeois" and "Marxist."

Is the distinctiveness of East German scholarship due solely to Marxism, its theoretical staple? While many Westerners joined with African scholars of the 1960s in retrieving history in an Afrocentric spirit and some have only lately begun in small numbers to study the economic penetration and peripheralization of the continent, the East Germans continuously pursued the European side of economic and political colonialism, particularly that of Imperial Germany. Their comparative advantage was enhanced by proximity and access to the Potsdam archives where Westerners and African scholars were either excluded or admitted only to be served with preselected files. Their disadvantage was and is in their isolation and relative inability to travel and exchange ideas in Africa or elsewhere. What is generally lacking in *Drang nach Afrika* is the excitement and sense of struggle that imbues left scholarship in France, Britain, or Africa.

The book moves chronologically from background to annexation (1884–85), to conquest and subjugation where that was direct or indirect, as with economic and political interests elsewhere (Morocco and South Africa), and finally to colonial regimes in the pre–World War I years. Germany occupies the forefront of the following chapters, which cover the colonial record as a whole, endeavors before the war to acquire more territory, the war itself, colonial "revisionism" in the Weimar period, fascist colonial aims and preparations, and the Second World War. Throughout, there is attention to the data for and interpretation of trade with the metro-

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pole and the interplay of economic interest groups with various official policies.

Students of former German colonies will be most impressed with Peter Sebald's discussion of Togo. Those with a conception of a "world system" will find abundant details and leads about what to take up if they get to see East German scholars and sources, but a framework for generalization about the relative place of Germany within the "core" of the industrialized world is lacking. Lastly, East Germans have not, on the evidence contained here, begun to think about modes of production and social formations within the colonized areas. Readers who follow the lively debates in this area will notice how inadequate is a narrowly channeled Marxism that does not incorporate such a perspective in approaches to imperialism and colonialism.

Drang nach Afrika is very much Helmuth Stoecker's book; he wrote almost half of it himself while also serving as editor. Yet it is meant to be a display of East German scholarship and primary source materials. A collective judgment is therefore in order, which must be that while much in the book is workmanlike, hardly any is pathbreaking. It will indeed be a pity if East German scholarship is not enabled in the 1980s to throw off its orthodoxy and join with the more vigorous Marxist historiography being practiced elsewhere.

MARCIA WRIGHT
Columbia University

LUCETTE VALENSI. *Fellahs tunisiens: L'économie rurale et la vie des campagnes aux 18e et 19e siècles.* (Civilisations et Sociétés, number 45.) Paris: Mouton. 1977. Pp. 421.

Between the masterly work of Robert Brunschvig and the careful studies of Jean Poncet and Jean Ganiage, there are three hundred and fifty years of Tunisian history about which little is known beyond dynastic chronology, skeletal political description, and military and corsair confrontation. Lucette Valensi, in a work to which she devoted more than a decade of painstaking research and thoughtful analysis, had originally hoped to fill that entire gap in an overarching study inspired and informed by the basic canons of the *Annales* school. As she explains in her preface, the first plan proved too ambitious; for the sixteenth and seventeenth centuries the sources were simply too sparse. What Valensi has finally produced is a study of rural Tunisia from the accession of the Husaynids to the Revolt of 1864. Tunisian scholars in particular and North Africanists in general will be grateful for this enormous contribution to the literature, both because of the period it covers and because of the kind of history it is.

The book is divided into three parts. The first deals with nomadic, seminomadic, and sedentary peoples of rural Tunisia—their histories, migrations, and their social organization. In a word, it deals with structures of the human groupings. The second part studies the adaptation of these groupings to the soil: forms of land tenure, crops and animal species raised, agricultural techniques, and patterns of exchange. The third part, following the *Annales* organizing pattern, comes closest to being an *histoire événementielle*. Here the author deals with the famines and epidemics, the economic currents, the foreign involvements, and the growing fiscal shortsightedness of the bey's government, which acted—negatively in the long run—upon the structures laid out in the first two parts.

To brashly simplify Valensi's view of Tunisia between 1805 and 1864, she advances the picture of a land peopled by largely self-sufficient agricultural producers eking out a frugal existence with occasionally some exportable surplus from a myriad of small plots whose proliferation and integrity reflect the basic structures of tribal and sedentary societies. A stable eighteenth century gave way to a nineteenth century in which Tunisia moved methodically down the path toward underdevelopment. From 1784 onward, plague, cholera, and famine brought about progressive depopulation, while the extractions of European capitalists and the beylical establishment itself placed the "stable" structures of the eighteenth century under increasing pressure. The result was socioeconomic regression and a series of revolts culminating in the great upheaval of 1864.

For all its great value, *Fellahs tunisiens* is not unflawed. The author herself recognizes that the rarity of appropriate series of numbers made it impossible to create a work in the pure tradition of a Braudel or Labrousse. Population, tax, price, and trade series were available in bewilderingly discontinuous and dissimilar forms. This is one of the reasons why the author frequently answers a question with more questions and why she has had to cover herself by posing as hypothesis matters that in other works would have stood as conclusions.

But *Fellahs tunisiens* has other problems—problems of symmetry and integration. After a long and creative chapter on the structure of nomadic and seminomadic groups, the author uses a few unconvincing and slimly documented paragraphs to inform us that the structure of village societies is nearly identical. These same villagers whose society is so sparingly studied were responsible for the largest part of the agricultural production of the Regency. The chapter on land tenure fails to come to grips with some of the basic issues land specialists concern themselves with. Finally, there is a disquieting sense of *déjà vu* in the relationship of part three, which deals with sequences of events, to parts one and two, which lay out the basic structures. In

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Braudel's model work on the Mediterranean, the second volume is supposed to make more sense as a result of the exhaustive analyses of volume one. I have never been convinced that it does. Valensi's structural sections should inform her events, while the latter should act upon the structures. In both directions the fundamental interrelationships are only partially explicit.

JOHN RUEDY
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MARTHA CRENSHAW HUTCHINSON. *Revolutionary Terrorism: The FLN in Algeria, 1954–1962.* Stanford, Calif.: Hoover Institution Press. 1978. Pp. xvi, 178. $10.95.

The rather considerable merit of Martha Crenshaw Hutchinson's short volume on the role of terror during the Algerian Revolution is that the author presents an unequivocal defense of a particular instance of an extreme form of political violence at a time when such views are not at all in fashion. To be sure, Hutchinson denies in her introduction that she has any such purpose—"the problem of the morality of violence is outside the scope of this study" (p. xvi)—yet her book is probably the most thorough political justification of terror during the Algerian Revolution yet to appear. (It must be emphasized that her argument has to do with the *political* function of terror: Hutchinson is not at all impressed with the putative psychologically therapeutic power of terrorism popularized by Fanon and Sartre.)

The bulk of this work attempts to categorize specific proximate ends to which revolutionary terrorism was a means: rallying the Muslim population to the insurrection; demoralizing and disorganizing the European population; gaining foreign support for the rebellion; and demonstrating to Paris the seriousness of the nationalist challenge in North Africa. Each of these ends in turn became means serving the ultimate end: Algerian national independence. Although it is true that Hutchinson is not concerned to examine the moral credentials of this final end (hence, perhaps, her belief that hers is not a form of casuistry), there can be little question that the implicit means-ends structure of her discussion is fundamental to any serious debate on the moral justification for revolutionary violence. Certainly this "hidden agenda" of the study is its most interesting aspect.

The book may be faulted in two respects. First, by failing to recognize explicitly the moral dimension of her study, Hutchinson misses the opportunity to compare her approach with that of other theorists on the matter (including not only such writers as Machiavelli and Trotsky, but also Algerian-born Camus, of whom only scant mention is made). Hutchinson compares other cases of terrorism to the Algerian situation, but the attempt to discriminate empirically among these cases has been carried much further by others. Why did she not attack the theoretical issues surrounding the justification of political violence more deliberately, since this is where the strength of the study lies? Secondly, the force of such a means-ends argument depends in considerable measure on the historical evidence assembled to sustain it. Hutchinson is persuasive that on balance terrorism served each of the ends mentioned above (and is careful to point out that its effects in each respect were not unambiguous). But at points she comes close to suggesting that terrorism was indispensable in the achievement of these ends (in conjunction, of course, with military and diplomatic efforts). Yet to make such an argument requires far more evidence than Hutchinson provides. Was it actually beyond the bounds of the historically possible, for example, that the incidence of terrorism might have been reduced within the Muslim community had the National Liberation Front (FLN) more thoroughly prepared the political terrain before launching the insurrection? Similar counterfactual questions could be asked of each of Hutchinson's categories of analysis. Hutchinson is clear that even for its own ends terrorism in Algeria risked being counterproductive, but her study is not full enough to examine whether other means might have served the same function as terror. If other revolutions (such as the Vietnamese) demonstrate that there is little probability terrorism can be dispensed with entirely, we nonetheless need far more information than this book provides to be convinced that terrorism on the scale it occurred in Algeria was necessary for national independence.

These criticisms are, however, relatively minor. While readers interested strictly in Algeria may find little new in these pages, those interested in comparative studies of revolutionary violence should find the book well worth their attention.

TONY SMITH
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WERNER PEUKERT. *Der Atlantische Sklavenhandel von Dahomey, 1740–1797: Wirtschaftsanthropologie und Sozialgeschichte.* (Studien zur Kulturkunde, number 40.) Wiesbaden: Franz Steiner Verlag. 1978. Pp. xvi, 412. DM 48.

Dahomey has figured in the older historiography of Africa as an outstanding example documenting the "Atlanticist" view of African historical development between the beginning of European contacts and the nineteenth century. According to this view, the Atlantic slave trade, foisted upon Africa and dominated in all its aspects by the European partners,

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was the major determinant of African economic and political developments during that period. It stifled other economic activities and impoverished the continent as a whole, while stimulating warfare and the development of predatory centralized slave-raiding states whose trade contact with Europeans provided them with guns and hence preponderant military power. A "slave-gun cycle" resulted.

In the field of economic anthropology, Karl Polanyi used a study of Dahomey to document his "substantivist" theory, according to which the "archaic" economic arrangements of early empires were *sui generis*, characterized by economic modes of redistribution and reciprocity rather than market mechanisms, and therefore not amenable to analysis through the categories of modern economic theory. While Polanyi's theory has been the subject of prolonged academic debate, his description and analysis of the particular case of Dahomey has not heretofore been challenged.

Werner Peukert's work is an explicit attack on both theories. His evidence is largely drawn from contemporary European traders' and travelers' reports, from published ships' account books, and from the unpublished accounts of the French and British trading forts in Whydah (Glehwe), which after its conquest by Dahomey in 1727 served as that kingdom's sole foreign trade center. He succeeds brilliantly in demonstrating, through ingenious quantitative analysis, that the main elements of a "modern" economic order existed in Dahomey: there was a high degree of economic specialization with attendant income differentials; prices were determined by market factors, rather than fixed by royal fiat, as Polanyi alleged. Trade, money, and markets were integrated, not separate, institutions.

The "Atlanticist" theory has already been undermined considerably over the last decade in specialized studies of the economic history of various areas of West Africa (for example, works by Curtin, Dike, Fage, Fyfe, and Wilks) that have tended to demonstrate that the trade's role in and impact on African societies was smaller, and the African side's ability to affect its terms and nature larger than in the traditional model. Nevertheless, Dahomey continues to be cited in general works as the classical example of a slave-raiding "gun state," whose institutions, policy, and socioeconomic order were largely determined by its involvement in the slave trade.

Peukert demonstrates convincingly that (1) the slave trade accounted for no more than 10 percent, and perhaps as little as 2.5 percent, of Dahomeyan "GNP"; (2) prices and mechanics of the trade were largely influenced by African determinants; (3) the role of war in slave recruitment was negligible, wars generally led to declines in slave exports by interrupting trade with the interior; (4) the slave trade of Whydah did not grow but declined as the result of its conquest by the warlike Dahomeyan monarchy; and (5) guns played a very minor part among the imports of the kingdom, whose military fortunes in the period were hardly splendid anyway. (One wonders why the notion of a decisive military advantage deriving from the possession of eighteenth-century trade muskets has not long been questioned on the grounds of the technical characteristics of such weapons alone!)

This is not an easy book to read. While written clearly enough, it makes its points with an unrelenting, systematic barrage of evidence and argument. We are told what we are going to be told, then told, then told what has been told. Abundant information on prices, trade goods and practices, import and export statistics, and much else is presented in lengthy appendixes. Peukert has made a superb contribution to a major question in African history which should encourage work on similar lines on other areas of the continent.

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DAVID NORTHRUP. *Trade Without Rulers: Pre-Colonial Economic Development in South-Eastern Nigeria.* (Oxford Studies in African Affairs.) Oxford: Clarendon Press. 1978. Pp. viii, 269. $23.50.

Recent studies that deal with trade and commerce in Africa have tended to concentrate on those areas whose commercial organizations were based on organized political kingdoms. The people of southeastern Nigeria, the focus of David Northrup's study, evolved no political structures beyond the local level. The study deals with the economic history of this important area from the period before the arrival of Europeans to the nineteenth century, when legitimate trade replaced slave trading. According to Northrup the thematic unity and the main thrust of the history of this area are clearly revealed through its economic history. Northrup is therefore concerned throughout the book with the institutions and personnel of its commercial organization, markets, regional and long-distance trade and trade routes, and trade goods and currencies.

Unlike many earlier studies on economic development in Africa, greater emphasis is placed on the interior trade, trade routes, and markets than on the coastal ports and the coastal zone through which the external ties were channeled. Thus trade, including the slave trade, is examined in terms of African rather than European trade. By this approach Northrup is able to provide a useful case study to the process of reinterpreting commercial development in southeastern Nigeria.

The bulk of Northrup's work is devoted to the slave trade. He traces the development and growth

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of the slave trade and determines the volume, origins, manner of enslavement, and demographic impact of the slave trade.

In the discussion of the slave trade Northrup seeks to follow in the tradition of what he regards as the serious studies of Philip Curtin, J. D. Fage, and A. G. Hopkins. The views of these scholars are not to be taken lightly, but there are points in the book where Northrup could have followed his own evidence to yield new insights. Northrup questions whether figures are in fact lacking for the slave trade in this area from 1711–30. But he accepts Curtin's figures from 1731 onwards even though he thinks that they are conservative estimates in view of the numbers known to be entering Virginia from the Bight of Biafra. Had he revised Curtin's figures based on his own research, even if he did not have J. E. Inokori's recent discussion when the book went to press, important new insights would have been yielded. Furthermore some of Northrup's conclusions, for instance on the small role of war in producing slaves, are not very convincing. Here he quotes the English slave ship surgeon Alexander Falconbridge who said that he never saw any slaves with recent wounds. If slaves indeed came from the interior through the regional and long-distance trade networks, it is unlikely that by the time they arrived on the coast there would be slaves with recent wounds. The fact that some slaves were also purchased from interior markets, as some of his sources suggest, does not obviate the possibility that these people were originally war prisoners. Again, Northrup's emphasis on the large number of kidnapped slaves overlooks the fact that many of the so-called wars were sometimes nothing but raids and kidnapping forays to obtain slaves. Finally, very few historians who work and live in southeastern Nigeria will accept his view that economically the slave trade in this area was not the disaster it is sometimes suggested to have been.

In spite of the above criticisms, Northrup's study is a work of considerable merit.

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ROBERT W. STRAYER. *The Making of Mission Communities in East Africa: Anglicans and Africans in Colonial Kenya, 1875–1935.* Albany: State University of New York Press. 1978. Pp. ix, 174. $30.00.

The history of Christianity and of Christian missions in Africa has inevitably been closely linked with wider trends in African historiography. As Robert W. Strayer points out in a fine historiographic introduction to *The Making of Mission Communities in East Africa*, the early focus on European strategies for implanting Christianity in Africa gave way to a nationalist tradition emphasizing African initiative. More recently, scholars have been concerned with African religious history and an examination of interaction between Africans and missions at the religious rather than political level as they have come to recognize the attraction of Christian symbols, techniques and mythology, the popular religious roots of Christian expansion, and the vitality of indigenous religion. What links these approaches are the two great facts about Christianity in Africa: its remarkable success and its divergence from European models.

The interest in independent churches, separatist movements, "revitalization," and millenarianism has overshadowed, however, the continuing vigor of orthodox mission communities despite competition and internal conflict. Strayer is thus concerned with the making of mission communities and the transformation and integration of mission stations into societies and therefore emphasizes the multiple interactions between Africans and English missionaries. Strayer sees his contribution to mission history as an examination of this process through a study of the Anglican Church Missionary Society in Kenya between 1875 and 1935 and its ramifications throughout modern Kenyan history. Although this topic is of interest in its own right, Strayer never presents a clear justification for focusing solely on the CMS or on this particular period. This is especially distressing in light of his prior plea for comparative studies in "Mission History in Africa: New Perspectives on an Encounter" (*African Studies Review*, 19 [1976]: 1–15). Indeed, the work is much strengthened when read in conjunction with his earlier article, which makes even more explicit the themes of adaptation, disappointment, and distribution of power.

Strayer ably handles these and other important issues such as the amplification of tension within the mission by its interaction with the larger colonial setting, the cohesion and continuity of mission churches, and the continued attractiveness of mainline churches despite independent movements. He explores the nuances of mission interaction with colonial politics, and his discussions of mission relationships with white settlers and with Harry Thuku are quite enlightening. The chapter written with Jocelyn Murray on the female circumcision controversy, which focused many tensions of colonial Kenya, is especially good. It re-emphasizes the theme of the "variety of policy, practice and impact within CMS communities and the tortuous process by which these various positions were reached" (p. 136).

Although Strayer is good on the complexities of certain questions, he fails to discuss adequately the implications of his insights, which too often are of-

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fered as conclusions and summaries of detail. The book unfortunately bears too many marks of the revised Ph.D. dissertation. And although Strayer interviewed almost three dozen people, he bases the final result heavily on archival material, with only rare citations to the interviews. Murray's oral work, on the other hand, contributes significantly to the section on circumcision. Despite these shortcomings, *The Making of Mission Communities* discusses important questions and makes a significant contribution to mission history. It clearly shows that mission communities are an important arena for the making of contemporary African culture.

JOHN ZARWAN
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JOHN A. MARCUM. *The Angolan Revolution.* Volume 2, *Exile Politics and Guerrilla Warfare (1962–1976).* (Studies in Communism, Revisionism, and Revolution, number 22.) Cambridge: MIT Press. 1978. Pp. xiv, 473. $30.00.

The revolution in Angola was perhaps one of the most widely reported and least understood revolutionary movements of the last twenty years. Published reports were frequently partisan and often confused. The array of acronyms and personalities, their positions on issues or on the battlefield were never very clear to either the public or to policy makers around the world. John A. Marcum's second volume on the Angolan revolution brings retrospective order to the chaos of our collective understanding. Further, though Marcum is a political scientist, he both shows his mastery of and pays tribute to the historical art. Although he makes a bow in the direction of developing a theoretical structure for the study of exile politics generally, his primary interest is in elucidating "the discrete, informal realities" (p. 280) of the Angolan situation, without the knowledge of which, he persuasively argues, policy makers in foreign capitals are doomed to perpetual miscalculation.

Angolan nationalist movements, their leaders, and the relations between them are narrated in great detail. An appendix entitled "A Partial List of Angolan Nationalist Movements—1962–1976" names no less than 87 organizations which claimed that title at one time or other during the period. Two sections form the body of the text. The first, entitled "The Pan African Phase," covers the years 1962–65. During this period exiled Angolan nationalist groups coalesced around the competition for OAU support between the Bakongo-based FNLA (National Front for the Liberation of Angola) under Holden Roberto, and the Mbundu-*mestiço*-based MPLA (Popular Movement for the Liberation of Angola) under Dr. Agostinho Neto. Section two, called "The Tripartite Phase," begins with the shift to three major (and numerous minor) competitors in the intricately linked struggles for military position, external support, personal fortune, and national liberation. It ends with the successful installation of an MPLA government in Luanda. Marcum stresses throughout the critical role of outsiders. He overlooks no one, from student support groups to Cuban troops.

The text is copiously annotated (125 pages of notes to 281 pages of text) and well indexed. Three appendixes provide an interesting counterpoint to the text and measurably increase the utility of the book for classroom use. The first appendix zeroes in on "Portugal's Colonial Mindset" through the medium of two speeches by Portuguese dictator António Salazar and one by his successor, Marcello Caetano. The second, "The Role of Contiguous States," is not as successful, but it does, through the vehicle of an official document each from the OAU and Zambia, succeed in conveying something of the role of those playing host to neighboring revolutionaries. The third contains a speech by Dr. Neto in early 1974 in which he clearly articulates a platform for exile liberation movements on such issues as how to know one's enemy and how to avoid the pitfalls of, among other things, racism and ideological squabbling.

This beautifully constructed diplomatic history is the first and last word on the exile roots of contemporary Angolan politics. Although there are times toward the middle of the book when the plethora of organizations and their shifting relationships threatens to overwhelm, Marcum never lets this happen. He does, however, whet the appetite for a third equally objective, well written, meticulously researched, and copiously annotated volume dealing with the internal organization, dynamics, and leadership of the major movements and the relationship of each to its constituencies within Angola. In the meantime this book is required reading for anyone interested in contemporary Angola in particular, in African affairs generally, or in exile politics wherever they occur.

SUSAN HERLIN BROADHEAD
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GAIL M. GERHART. *Black Power in South Africa: The Evolution of an Ideology.* (Perspectives on Southern Africa, number 19.) Berkeley and Los Angeles: University of California Press. 1978. Pp. xi, 364. $14.50.

In June 1976, the black township of Soweto outside Johannesburg exploded in anger; white police opened fire and some five hundred people—mainly black schoolchildren—were killed. In the wake of

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this, townships all over South Africa erupted, and several thousand blacks—African, Coloured, and Asian—were arrested by the South African state. These momentous events provide both the point of departure and the concluding chapter for Gail M. Gerhart's important book on the evolution of an ideology of black power in South Africa and the transformation of black consciousness that she believes these events betokened.

In view of the dearth of serious and scholarly work on African political organization and thought in South Africa since World War II, Gerhart's book is more than welcome. The author traces in detail the ideological antecedents to recent events in the formation of the Youth League among members of the oldest African nationalist organization in South Africa—the African National Congress—and shows the continuities in the thought of the leaders of the Youth League like Lembede and Mda, with that of Robert Sobukwe, the leading ideologue of the Pan Africanists who broke away from the ANC in 1959, and the late Steve Biko, more recent leader of the Black Consciousness movement, who died in a South African jail last year. Indeed, the best chapters of the book, and its central core, deal with these four major leaders. Based on a wide range of written materials as well as on extensive oral evidence, her work has a sureness of touch here, and a sensitivity and balance that are rare qualities in a field that has too often been the monopoly of polemicists.

This said, it is perhaps unfair to complain. Nevertheless, Gerhart's understanding of the ideas of the leaders she has chosen to study does not extend fully either to the context in which they operated or to their followers. The first two chapters of her book appear to suggest that until 1943, when the Youth Leaguers appeared to their rescue, the African peasants were parochial, the workers confused, and the petty bourgeoisie at best naive, at worst stupid as well. These generalizations result from too heavy a leaning on the sweeping statements of superficial sociological surveys rather than detailed historical research. And while, for the period she has researched, Gerhart has a sympathetic perception of the position and problems of the leadership, the masses still remain to be manipulated at will, a backdrop against which real history is acted out. There is little exploration of the interface between leaders and their constituency, and little detailed analysis of the changing nature of the South African political economy over these years that would enable us to set the leaders and their ideology in context. As a result, although the work achieves its more limited goal and gives us a fine account of the ideology of black power (though black is largely portrayed as African rather than African, Coloured, and Indian—increasingly the usage within South Africa), it does not in the end go on to explain either Soweto 1976 or the path of future change in South Africa.

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ASIA AND THE EAST

DAVID N. KEIGHTLEY. *Sources of Shang History: The Oracle-Bone Inscriptions of Bronze Age China.* Berkeley and Los Angeles: University of California Press. 1978. Pp. xvii, 281. $25.00.

A new era in the study of early Chinese history is dawning, and David N. Keightley's impressive new book is a harbinger of the knowledge to come. The Shang dynasty (1500?–1050? B.C.) has been a most elusive period in China's past. It was only in the late 1920s, when excavations were undertaken at Anyang, that the Shang passed from the realm of legend into that of authenticated history. In the years since, our efforts to understand behavior in Shang times and, thereby, the origin of patterns that became characteristic in Chinese civilization have been hampered by the great difficulties involved in studying the Shang oracle bone inscriptions. The bones have been scattered in public and private collections throughout the world. Faked inscriptions abound. Many inscriptions are on bone fragments that too often have not been reassembled or have been incorrectly pieced together. Worst of all, virtually every word surviving in the inscriptions has presented a puzzle to those trying to determine etymologies, grammatical usages, and meanings.

In spite of a massive outpouring of literature on the inscriptions, it was not until the mid-1960s that some order began to appear in the chaos of oracle bone studies. Two works of that time stand out. One is a dictionary, the *Chia-ku wen-tzu chi-shih*, by the Chinese scholar Li Hsiao-ting, which collates commentaries on Shang graphs and attempts to arrive at word definitions. The other, *Inkyo bokuji sōrui*, by the Japanese specialist Shima Kunio, is a concordance of published inscriptions, arranged according to content and chronology. Neither book, of course, did more than make the field more accessible to students already highly trained in modern Asian languages and in Chinese historical linguistics, scholars who often worked in the fashion of traditional classicists rather than as historians.

Keightley's current work represents both a continuation of the process of synthesizing the scattered research of earlier scholars and a new step toward the ultimate goal of making historical sense out of our legacy of Shang texts. It is a meticulously pre-

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pared handbook covering all the basic aspects of oracle bone study: types of bones used, techniques of bone preparation, divination procedures, methods of writing, characteristic types of inscriptions, instructions on ways of authenticating, dating, and reading the inscriptions, and an appreciation of the Shang texts as historical sources. Copious notes refer the reader to the previous literature on each point raised and indicate current controversies and directions for future research. The work's value is much enhanced by extensive illustrations, tabulations of data, bibliographic citations, and a detailed index. The book is a model of historiographical comprehensiveness and rigor that no student of the Shang period can afford to do without. Henceforth, the obstacles to specialization in Shang history should be less daunting. We should be able to look forward not only to Keightley's promised *Studies in Shang Divination* but also to other historical examinations of this seminal era of the Chinese past.

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ARTHUR F. WRIGHT. *The Sui Dynasty.* Conclusion by ROBERT M. SOMERS. New York: Alfred A. Knopf. 1978. Pp. 237. $15.00.

This work was not written for the specialist, but it should be read by historians of China and particularly by historians of early China. It is one of those lamentably rare attempts to write the history of a Chinese age. The general reader will find this Sui history presented in an elegant manner and peopled with lively historical figures, for the late Arthur F. Wright eschewed social science history and concentrated on the individuals who made history. The specialist will appreciate the synthetic nature of this work, which pulls together and goes beyond Wright's earlier studies of Sui history.

The first two chapters are both background. We are introduced to the Chinese models the Sui rulers had before them and to Charlemagne for comparative purposes (although this comparison is not sustained in the work). Wright acquaints us with his major sources and reveals his frustrations generated by working with them. The second chapter provides a sweeping overview of sixth-century China. This chapter sketches the factors that divided north and south at the end of three centuries of disunion and also sets forth the notions that were shared between the Chinese south and the semi-Chinese north. There is, however, only minimal reference to the social and economic history of the period.

Chapter three, "The Rise of Yang Chien," is Wright at his best. Yang Chien, posthumously entitled Emperor Wen, was a nervous bundle of contradictions. Although a devout Buddhist who came closer than any Chinese ruler to establishing a Buddhist regime in China, Yang Chien was not deterred by his faith from personally beating officials to death in the halls of the palace. Although a thoroughly authoritarian ruler, he was continuously beset by doubts about himself and his regime. Wright describes this dynast and his immediate coterie with a fine feeling for the period and the commanding figures of it.

"Reunification" (chapter four) provides the only sustained institutional analysis in the work. The largest section of this chapter deals with the building of Ta-hsing-ch'eng, the Sui capital; Wright is again on familiar ground with this topic and he does an outstanding job of conveying the emerging majesty of this city, which, within a century, was to become the largest metropolitan center in the world. Other sections of this chapter describe central and local government reforms and recruitment mechanisms.

"The Restoration of Cultural Hegemony" (chapter five) complements the preceding chapter in that it deals with ritual and ritual codification, the new law code, educational institutions, Confucianism, Buddhism, and Taoism. Confucianism, however, is not adequately dealt with. We are told that different versions and interpretations of the Confucian classics were much debated in the Sui (p. 120), but the exemplary biography of Liu Cho, a renowned Confucian scholar, does not provide us with a sense of what issues were being discussed among the Confucianists. Similarly, we are offered a keen appreciation of the Buddhist commitment and fervor of Yang Chien, but we are not given much sense of what constituted the dominant strains of Buddhism.

The sixth chapter, a straightforward account of the conquest of the south, is the last chapter written for this volume. Chapters seven, eight, and nine come from a shorter history of the Sui written by Wright for *The Cambridge History of China.* The scale of the work changes sharply at this point; the average length of each of these three chapters is only about one-half the length of the average of the first six. Although shorter, these chapters follow the same general format. Thus, chapter seven parallels chapter three in providing a biography of the second Sui emperor followed by sketches of a dozen men, most of whom were members of the inner circle of the Sui court. Chapter eight, "The Dynasty at Its Height" takes up the second emperor's reforms (of the law code, for example) and the dynasty's construction projects (such as the secondary capital at Loyang and the canal system). Wright's final chapter ("Military Disaster and Political Collapse") outlines the wars of expansion of the Sui, wars culminating in the terribly costly attacks on

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Koguryō. The end of the dynasty is tersely handled, and Wright finishes with a review of the moral assessments of the last Sui emperor and a reminder of the importance of the Sui as a great unifying dynasty.

Robert M. Somers has written the final chapter, "The Sui Legacy," which is a combined summary of the work and projection forward into the T'ang period. He notes that the "equal field" system of land distribution and the *fu-ping* militia system were both carried over as part of the Sui legacy. Since both institutions were of immense importance in restoring and maintaining central power, he is wise to draw attention to them; in the course of the book, however, the first is only alluded to and the second not mentioned at all. Somers focuses attention on the differences in imperial style between the Sui and the T'ang emperors. It is precisely in the area of imperial style that Wright has made his major contribution. Through his biographical sketches and his generally well-chosen anecdotes he has demonstrated the importance of palace politics. Although Chinese historians are inclined to dwell on the Chinese imperial bureaucracy, the patrimonial ruler and his fears, jealousies, hatreds, and favoritisms were often far more important. That was certainly the case, as Wright makes clear, in the Sui period.

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PRESTON M. TORBERT. *The Ch'ing Imperial Household Department: A Study of its Organization and Principal Functions, 1662–1796.* (Harvard East Asian Monographs, number 71.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1977. Pp. xii, 267. $15.00.

Who served the Manchu emperors and controlled the palace eunuchs? Our vague notions are now replaced by solid research, originally under the dissertational advice of P'ing-ti Ho, which puts together published archives and works in Chinese, Japanese, and Western languages. Given more time and scope, one may suggest, the author might have added more information from miscellaneous Ch'ing-era writings, for their record of illuminating cases would have made the story more lively.

The Imperial Household Department, staffed by neither slaves, nor chattel, nor eunuchs but by Manchu bondservants under a form of indenture, was a misnomer. Besides serving imperial and palace needs, the department, a subbureaucracy by itself, had fifty-six major offices that managed, among other things, landed estates, workshops, textile production, and the like, as well as schools, the famous imperial press, and a book bindery. In fact, it exclusively managed the ruler's private treasury that paralleled several key areas of the government finance, such as tributes, customs, the salt monopoly, and the trade in copper, ginseng, and other goods.

The Manchu innovatively rejected a notorious feature of Chinese institutions: the eunuchs' abuse and usurpation of power, which become worse under the preceeding Ming dynasty. Drawing on its own non-Chinese sociomilitary background in Manchuria, as in the case of an earlier Manchu empire of the twelfth century, Manchu China used these loyal and relatively incorrupt bondservants to control the palace eunuchs, who were in turn given a measure of self-esteem.

Although these imperial agents generally avoided political abuses, economic corruption soon reappeared amidst eighteenth-century peace and prosperity. The author makes the point that this Manchu institution aided increasing autocracy. I agree, but there were far greater perspectives: first, this department collected, formally and informally, much wealth from the country, just as a colonial office does; second, it hardly came under the administrative checks-and-balances of the regular Confucian bureaucracy or government.

It is also regrettable that this excellent work gives but little emphasis to Ho-shen, a bondservant picked by the long-reigning Emperor Ch'ien-lung to be the most powerful court minister. Manchu China never recovered from his most demoralizing notoriety. Given the splendid groundwork now provided by Preston M. Torbert, someone should study this minister's career, not so much as a biographical study of Ho-shen, but as an investigation of a turning point in the history and institutions of Ch'ing China. One may also suggest a comparative study in Chinese history itself. Sung China (960–1279) was often praised for its generally effective control of eunuchs. What was its means of institutional control? Why was it ignored under the succeeding Mongols, in Ming China, and under the Manchus? Why did the Confucian scholar-officials remain largely silent?

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RICHARD J. SMITH. *Mercenaries and Mandarins: The Ever-Victorious Army in Nineteenth Century China.* (KTO Studies in American History.) Foreword by JOHN K. FAIRBANK. Millwood, N.Y.: KTO Press. 1978. Pp. xxiii, 271. $19.95.

The Ever-Victorious Army and its colorful commanders—the American Frederick T. Ward and the Englishman Charles "Chinese" Gordon—have long attracted the romantic interest of popular writers, who have played on the fascination inher-

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ent in the great Taiping Rebellion and Sino-Western interaction in mid-nineteenth-century China. But most of this interest has been characterized more by imagination and polemics based entirely on Western sources than by solid historical research in the documentary record of both sides. With Richard J. Smith's study, at last we have a systematic and critical examination of the origins, structure, role, and significance of the Ever-Victorious Army.

Having first established the context, expanding domestic rebellion and the threat it posed to the new order of Western commercial and political privileges centered on the strategic city of Shanghai, Smith describes the emergence of cooperative Sino-Western measures to defend the city against the Taiping offensive in 1860. Discussing at length the politics of intervention, Smith shows how Chinese deliberations concerning the wisdom of borrowing foreign troops, the debate over intervention between British policy makers in London and Peking and British commanders at Shanghai, and the concern with the problem of Anglo-French rivalry in China all focused on the Shanghai Foreign Arms Corps created by the Salem adventurer Frederick T. Ward. By mid-1862 British ambivalence toward intervention had been resolved in the decision to support the newly styled Ever-Victorious Army. At the same time, the early unreliability of the force had been largely overcome with its reorganization to employ Chinese troops trained in Western drill and tactics.

In subsequent chapters Smith examines the structure of the Ever-Victorious Army, the transition to British command under Gordon following Ward's death in battle, Gordon's unsuccessful attempts to reform the force, Gordon's role as a foreign advisor, and the significance of the Ever-Victorious Army for China's military modernization.

Throughout his analysis Smith draws upon John K. Fairbank's seminal thesis of "synarchy"—joint Chinese and "barbarian" participation in Chinese civil and military administrations—as a potential basis not only for preserving China against the threat of domestic upheaval but also for more consequential institutional reform on Western terms. But Smith shows that the hybrid character of the Ever-Victorious Army imposed inherent limitations on its effect, either as a model for military reform or as an instrument in the suppression of rebellion. Chinese and Western purposes converged originally in the deployment of the Ever-Victorious Army in the immediate defense of Shanghai, but once its possibilities expanded geographically and institutionally, Chinese and Western intentions parted ways, as the Chinese shrank back from the larger political and cultural implications in Western designs for the force. Although Western expectations for reform were disappointed, the Ever-Victorious Army did provide a limited conduit for the transmission of Western technology—mainly artillery and steamships—that China saw as its principal benefits. Yet in the long run synarchy had the effect of channeling and confining Western influence rather than promoting and expanding it.

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ODORIC Y. K. WOU. *Militarism in Modern China: The Career of Wu P'ei-Fu, 1916–39.* (Studies of the East Asian Institute, Columbia University.) Dawson: Australian National University Press; distributed by Archon Books, Hamden, Conn. 1978. Pp. 349. $22.50.

This biography of Wu P'ei-fu (1874–1939) is a welcome addition to the growing literature on China's warlord period (1916–28). Wu, who belongs to James Sheridan's category of "conservative" warlords—as contrasted with "reactionary" and "reformist" types—was the mainstay of the Chihli clique that controlled the Peking government from 1920 to 1924. He reached the zenith of personal power and prestige in 1922 when he set out to unify the country by forde. Two years later, defeat by the Manchurian satrap, Chang Tso-lin, sent him skidding downwards. Chiang Kai-shek's Northern Expedition (1926–27) finished his career.

Wu's spectacular rise had been a product of the breakdown of the traditional Confucian system. The son of a poor shopkeeper, he had studied the classics and obtained the *sheng-yüan* degree—the first rung in the traditional examination ladder. But his choice of a military career in 1898 reflected its growing importance as an avenue for mobility. In 1906 he was an officer in the third division, the crack unit of Yuan Shih-k'ai's Peiyang army, China's best fighting force. Hard work and loyalty to his superiors brought rapid promotion. Wu probably became China's finest military strategist of the time. In 1918 he was in command of the third division, with the title of "awe-inspiring general." In 1922 he was widely hailed as the "hope of China." The Russians and the Chinese Communists wooed him, and Europeans welcomed him as the strong man that China had been waiting for. Then, in November, Wu's cronies wrecked the "Able Men Cabinet," the best that China had during this period. In February 1923 his troops massacred striking railway workers—the Peking-Hankow line was his main strategic and financial asset—and later in the year he allowed his nominal superior, Ts'ao K'un, to buy the presidency of the republic. Wu was now seen as just another "military mandarin."

Lacking the imagination to overcome the system

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that had made him, Wu became its victim. Despite personal honesty and patriotism, he, like other warlords, depended solely upon coercive power. He had no political ideology, only a sentimental attachment to Confucian morality, and no permanent civilian constituency, only a loose, clan-like cluster of generals who were never more restless than when the time came to divide the spoils. And despite extortionate taxation, the opium trade, and forced loans from businessmen, there was never enough to satisfy them all.

Yet, as the author shows, Wu was not an imperialist stooge. He did try to get British help—his enemies were getting it from Japan and Russia—but the Foreign Office chose a hands-off policy. Before he died Wu had the satisfaction of exasperating General Doihara and other China "experts" who wanted him to head Japan's puppet government in China. He told them that Japan should pay humble respect to "elder brother" China and evacuate its troops. And, instead of conquering territory to solve their population problem, Wu advised the Japanese to try birth control.

Odoric Y. K. Wou's approach, thematic rather than chronological, poses some difficulty in keeping track of actors and events during this highly fluid, if not anarchic, period. But it fits his purpose of analyzing the warlord system as well as Wu's rise and fall. The book is divided into two parts—the first dealing with internal politics, and the second, with foreign relations. Both supply rich and well-documented material.

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GUY S. ALITTO. *The Last Confucian: Liang Shu-ming and the Chinese Dilemma of Modernity.* Berkeley and Los Angeles: University of California Press, for the Center for Chinese Studies. 1979. Pp. xvii, 396. $17.50.

Liang Shu-ming, born in 1893, has hitherto appeared as a peripheral figure in studies of the intellectual and social history of twentieth-century China. In this perceptive biography, Guy S. Alitto gives Liang the attention he deserves in his own right and adds substantially to our understanding of the range of motives that impelled men to intellectual commitment and social engagement in China's chaotic republican decades.

Liang was (or is) the last of a line of Confucian scholar-aristocrats that originated in the fourteenth century. His father, Liang Chi, a Confucian of uncommonly austere personal conviction, was at the same time one of the more progressive constitutionalist reformers during the last years of the dynasty, which he served in a minor bureaucratic capacity. Alitto speculates with judicious circumspection on the psychological implications of the father-son relationship. What is certain is that Liang Chi's suicide in 1918, an act of moral remonstrance against the degeneracy of the age, was a direct cause of his son's "conversion" to Confucianism. Liang Shu-ming had been, until 1912, an active revolutionary; in the unhappy early republican years he retired, spiritually depleted, into Buddhism. In the May Fourth period he taught Buddhist studies at Peking University, where he came into contact with many of the radical "Westernizers" who were his destined adversaries. In the early 1920s Liang emerged in the role in which he is best known, as an eloquent and subtle defender of Confucianism's "humanistic" values against the claims of intellectual modernizers whose rationalist-materialist ideas and self-indulgent life style he despised.

Alitto's treatment of the "Eastern versus Western culture" controversies of the 1920s and 1930s is well balanced: sympathetic to his protagonist's position, but fully aware of its inconsistencies. Liang desired to maintain China's unique "spiritual" heritage while at the same time preserving China's communal life—which meant, willy-nilly, maintaining or creating a Chinese state. Liang's vision transcended political and historical limitations. He was the self-appointed transmitter of the ancient way (hence the claim of the book's title: *The Last Confucian*), a messianic culturalist, not a nationalist. But the exigencies of government intruded decisively.

The most important chapters of Alitto's book concern Liang's activities as a social reformer, most notably his engagement in rural reconstruction in Shantung in the 1930s. Liang's efforts to realize the ideals of Confucian moral agrarianism resulted in the development, from entirely indigenous sources, of social strategies that resembled in important respects those of the Yenan Communists. There was the same emphasis on small-group dynamics as a leadership technique; on collective moral self-awareness, mutual responsibility, and criticism; on eliminating, or at least minimizing, the gap between intellectual/bureaucratic leadership on the one hand and "mass" interests on the other. A lietmotif running throughout the book is the chronicle of infrequent but provocative confrontations between Liang, the orthodox Confucian, and Mao, the unorthodox Marxist, men who "shared a bone-deep Chineseness, which, in the end, made them closer to each other than to many Westernized Chinese liberals and members of either the KMT or CCP" (p. 286).

Alitto comes close to describing Liang's social program as "Maoism without class struggle." And, one might add, without nationalism. One wonders how important the omissions would be at the level of village life. One wonders, too, whether the rigid moral authoritarianism of Liangist Confucianism-

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in-action would have been any more tolerant of individuality than was the ideological authoritarianism of Maoist communism.

In respect to its animating ideas and its social and political involvments, Liang's was a well-documented life. Alitto's study exploits this fact to excellent advantage. Although his handling of Liang's critics (or the objects of Liang's criticism) seems often two-dimensional and monochromatic, the portrait of Liang himself is colorful, compelling, and persuasive. Alitto possesses the skills that distinguish the good biographer: the capacity to empathize with his subject without sacrificing his own critical sense and the wisdom to let the coherence of the life count for more, in the end, than the contradictions of the ideas.

This is a history of ideas being lived. It should be required reading for serious students of modern China. No one who reads it will be able again to dismiss "Confucian traditionalism" as a stereotypical and anachronistic response to the "dilemma of modernity."

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LIN YU-SHENG. *The Crisis of Chinese Consciousness: Radical Antitraditionalism in the May Fourth Era.* Foreword by BENJAMIN I. SCHWARTZ. Madison: University of Wisconsin Press. 1979. Pp. xiv, 201. $20.00.

Ever since modern Chinese intellectual history emerged as a distinctive field of scholarly inquiry it has centered on the problem of conflict between traditional and modern ideas. This problem is complicated by the fact that most modern ideas were foreign, and the intellectuals who subscribed to them were also intense patriots, active in antiforeign political movements. Their anti-imperialist nationalism was primarily a modern and foreign-inspired sentiment, but it had a traditional dimension, a certain deep cultural pride that stemmed from the social and educational backgrounds of these intellectuals. Most came from traditional literati, upper-class families and had old-fashioned classical training, although many had also traveled and studied abroad. Not surprisingly, therefore, scholarship on modern Chinese intellectual history is full of references to tension, irony, ambiguity, paradox, and contradiction.

Scholars are repeatedly drawn back to the period when the modern Chinese intelligentsia became a powerful force and its problems crystallized, roughly the decade 1915–25, pivoting on the May 4, 1919, demonstrations and the larger nationalist and "new culture" movements that May Fourth represented. Lin Yü-sheng has already addressed the period and some of the central issues in several outstanding articles. Now we have his first book, a revision of his dissertation, which explores yet another paradox, one which others have touched upon but none have examined in depth. To put it too simply, it is that the very intellectuals who led the radical anti-traditionalism of the May Fourth era were themselves, albeit for the most part unconsciously, too traditional to resolve "the crisis of Chinese consciousness." That crisis, in Lin's view, was created by the onslaught of the West and the disintegration of the traditional Chinese order, in response to which Chinese intellectuals adopted an "iconoclastic totalism": they declared "permanent war against Chinese tradition." But this all-out crusade was self-defeating, because "the question of national identity could not be resolved by a militant negation of the nation's past. On the contrary, national identity calls for some sort of positive relationship with that past" (p. 155). The final irony, and Lin's major hypothesis, is that the radical intellectuals' self-defeating "totalism" was itself traditional: it was "a deep-seated traditional Chinese cultural predisposition" both to view intellectual and cultural change as the essential precondition and foundation for all other changes and to insist that Chinese society was so organic a whole that it was impossible to change only parts of it. The radical Chinese intellectuals' antitraditionalism was "decisively molded" by tradition without their being aware of it (pp. 28, 50).

The first third of Lin's book explicates the foregoing thesis in broad strokes, ranging back to the second millenium B.C. for evidence from Shang oracle inscriptions and drawing upon major thinkers from Confucius to Liang Ch'i-ch'ao. The thesis is then tested in three studies of individuals—Ch'en Tu-hsiu, Hu Shih, and, especially, Lu Hsun. The method and the choice of individuals make sense, but the execution is not all that it might be. The author has tried to do a great deal in rather a short book—the text, minus the many lengthy footnotes, amounts only to about one hundred thirty pages—and too much remains either unexplained or obscured by dissertation style. How can Ch'en, who had accepted Marxism by 1920, be treated simply as a wholehearted believer in the monist "cultural-intellectualistic approach" with no reference to dialectical materialism? Hu's commitment to John Dewey's "instrumentalism" poses similar problems that I do not think are satisfactorily resolved here, as do the conscious high regard that Hu and Lu Hsun had for elements of Chinese tradition. Still, parts of the analysis are richly rewarding, especially in the chapter on Lu Hsun: the thesis is stimulating; and the book is informed by a wealth of learning. Specialists will learn a good deal from it.

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Z. D. KATKOVA. *Vneshniaia politika gomin'danovskogo pravitel'stva Kitaia v period antiiaponskoi voiny (1937–1945)* [The Foreign Policy of the Kuomintang Government of China during the Anti-Japanese War (1937–45)]. Moscow: Izdatel'stvo "Nauka." 1978. Pp. 239. 1 r. 30 k.

Dealing with China's foreign policy during its eight-year war of resistance against Japan, Z. D. Katkova's book is equipped with a simple chronological framework. After a description of the international situation and China's foreign relations on the eve of that war, the author discusses the foreign policy of the Nationalist government from July 7, 1937 to the end of World War II in three periods, divided by the outbreak of the European (September 1, 1939) and the Pacific (December 7, 1941) wars. The book analyzes Chiang Kai-shek's foreign policy "programs" and their realization by "China's ruling circles."

Indeed, the author is compelled to conclude that, in spite of contradictions in China's foreign policy lines, its performance "in a united front with countries of the antifascist coalition played an important role in the destruction of the fascist bloc of aggressors in the course of the Second World War" (p. 224). Yet the author's exceedingly low opinion of the Chiang government prevails throughout the book. According to her, China, because of "economic, political and military weaknesses," was "unable to cope with Japan by its own strengths" but "relied more heavily on imperialist countries of the West and the USA" (p. 222).

Katkova's liberal downgrading of China's Nationalist regime applies to its ability as well as its integrity. On the one hand, she acknowledges that China's war of resistance against Japan "carried the just national-liberation character" of "defending its freedom and independence" (p. 222). On the other hand, she denounces "the most evident nationalism and chauvinism of the Kuomintang government" in "playing the role of an ally of the USA and England" not only "to recover lost territories" but also, according to "Chinese . . . tradition," "to establish China's hegemony over certain Asiatic countries after the conclusion of the war" (p. 223). This vilification, however, is overshadowed by the author's endeavor to belittle China's determination to continue the war, especially before the beginning of the war in the Pacific. She deliberates at length on the alleged peace talks proposed by Japan even up to early 1945, when Chiang supposedly gave up any further search for compromise with Japan.

The author's indulgence in insinuation against China is matched only by her unjustified exaltation of the Soviet role in ending the war with Japan, the atomic bombing of Hiroshima and Nagasaki notwithstanding. The author's assertion misrepresents history, as Japan was ready to surrender following Hiroshima and Nagasaki, and the USSR rushed into war in order to grasp the victory at hand and the enormous spoils acquired by promise at Yalta. In fact, the USSR could not wait, as originally intended, until the signing a week later of the Treaty of Friendship and Alliance with the Nationalist regime of China, which is surprisingly not referred to in the book, as the prerequisite to entering the war by marching into Manchuria. Ironically, as this writer witnessed first hand, the 1945 Sino-Soviet treaty was actually signed on August 15 but dated August 14, the day of Japanese surrender, and announced two hours before the broadcast of the news of Japanese surrender. Thus, the book is further evidence of scholarship in service to the Soviet purpose rather than a genuine contribution.

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MORINOSUKE KAJIMA. *The Diplomacy of Japan, 1894–1922.* Volume 2, *Anglo-Japanese Alliance and Russo-Japanese War.* Tokyo: Kajima Institute of International Peace; distributed by Kajima Institute Publishing. 1978. Pp. 523.

Like volume one of the *Diplomacy of Japan* (*AHR*, 83 [1978]:787), this book is essentially a collection of documents. Focusing on the years 1901–05, it chronicles Japan's relations with the powers in terms of Japanese official documents. The scope is extremely limited: the book deals almost entirely with government-to-government relations, with little attempt made to discuss the interplay between foreign policy and domestic politics. There is hardly any discussion of economic factors except insofar as they are mentioned in official dispatches. The bulk of the book is devoted to Japanese negotiations with Britain for an alliance (and for its renewal) and with Russia preceding and following the Russo-Japanese War. There is very little about other countries, such as the United States, China, and Korea.

Morinosuke Kajima's presentation is Japan-centered. Because the book reproduces countless Japanese official documents as the sole basis of its narrative, this is not surprising. But, reading the book, one would never know that the "diplomacy" of Japan involved other issues such as commercial expansion in areas other than Manchuria or the treatment of Chinese students in Japan or that there was an intensive debate about imperialism within as well as outside the country at that time. On the origins of the war with Russia, the book simply repeats the official Japanese line (for example, that the Russian government had "no sincere intention of compromising" with Tokyo [p. 97]).

Is there merit in an English publication of such a book? The book does translate a vast number of

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Japanese documents into English, making available for the first time in English, for instance, the proceedings of the Portsmouth Peace Conference as recorded by the Japanese. There are in fact over two hundred pages of these proceedings. But merely to reproduce a document is not enough. Instead of reprinting, for example, the Japanese government's January 1905 statement that "the full maintenance of Japan's preponderant influence in Korea is necessary and indispensable for Japan's peace and security" (p. 205), the author should have discussed how such a perception emerged and why it was shared by governmental officials. Needless to say, such a statement must, if its validity were to be analyzed, be examined in the context of Korean as well as Japanese politics.

The book makes no attempt to use the monographic literature written during the past fifty years; its secondary sources are limited mostly to publications of and before the 1920s. Those who are familiar with the historiography of the last fifty years will find nothing new in this book.

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SABURŌ IENAGA. *The Pacific War: World War II and the Japanese, 1931-1945.* Translated by FRANK BALDWIN. (The Pantheon Asia Library.) New York: Pantheon Books. 1978. Pp. xvi, 316. $10.00.

In his book, *The Pacific War: World War II and the Japanese, 1931-1945*, the eminent Japanese intellectual and cultural historian, Saburō Ienaga presents a thoughtful and concise overview of Japan's role in World War II. Ienaga's purpose in publishing the book in Japanese in 1968 was to show the Japanese people the "naked realities" of the Pacific War and "to stimulate reflection and self criticism about the war" (p. xi). Ienaga wanted to demystify war, to show its horrors so that those Japanese, and especially those in the young, postwar generation, who glorified war as a time of heroism, might see its grim realities. Then Ienaga authorized the translation of his book into English in order to reach an even larger audience. As Frank Baldwin states in his translator's preface, many Japanese, including Ienaga, believe that the American involvement in Indochina was similar to the Japanese experience in World War II; thus, Westerners also can benefit from the lessons of the Pacific War.

To achieve his educational goal, Ienaga has written a social, rather than a political and diplomatic, history of the war. He emphasizes not international negotiations, coups d'état, battles, and incidents at the leaders' level, but the effects of the war on Japanese, Chinese, Koreans, and South and Southeast Asians in general. His conclusion is that the war was devastating; millions died, lost their homes and their livelihood, and suffered incredible hardships. By way of illustration, the author presents anecdote after anecdote describing the horrible impact of the war on combatants and civilians alike. The evidence is overwhelming; even those readers who demand the most compelling statistical evidence will come away from a reading of Ienaga's chapter entitled "The Horrors of the War" (chapter nine) convinced that the Japanese perpetrated unspeakable atrocities during the fifteen years of the Pacific War.

The humiliation heaped on other Asians was psychological as well as physical. Ienaga shows also how his countrymen attempted to rob their Korean and Taiwanese neighbors of their livelihoods, their cultures, and finally their very identities. Japanese occupation policy required them to use the Japanese language in schools, to worship at Shinto shrines, and finally to adopt Japanese names. These demands, in addition to the Japan-centered ideology of the Greater East Asia Co-Prosperity Sphere, put the lie to the claims of contemporary and later Japanese "Pan-Asianists" that the Pacific War was one fought by all Asians together to rid the Far East of Western imperialism.

But it was not only Koreans, Chinese, and other Asians who suffered. The war also destroyed any semblance of democracy at home (chapter six), and then, in its final months, rained physical destruction on the home country as well. American attacks with incendiary bombs on dozens of Japanese cities—80,000 people died in Tokyo on the night of March 10, 1945, alone—and with atomic bombs on Hiroshima and Nagasaki killed hundreds of thousands of people and destroyed the homes and property of millions of others. In August 1945, one could look across Tokyo for miles without a building impeding one's view.

The author is not satisfied to describe only the costs of the war at home and abroad. He also attempts to explain why "the war was not prevented," and this is both the book's most intriguing and ultimately most disappointing part. Ienaga divides his section on the reasons for the war into three chapters: one each on Japanese racist attitudes toward Chinese and Koreans, on thought control and educational indoctrination at home, and on the irrationality of the Japanese military. But, although Ienaga touches all of the causative bases, he does not treat the reasons for war in the depth with which he describes the war itself. Part two, on the "conduct of the war," occupies one hundred eighty pages; part one, entitled, "Why was the war not prevented?" only fifty. Moreover, Ienaga refrains from attempting any psychological explanations of the wartime behavior of the Japanese, although at least two provocative models, those of Ruth Benedict and Maruyama Masao, were available to him.

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But the sketchiness of Ienaga's discussion of Japanese racism, thought control, indoctrination, and military irrationality does not vitiate this book's contribution to our understanding of modern Japan. It is intended as an overview, and it is a provocative and moving one. For those readers interested in pursuing the causal questions in greater depth, I refer them to such people as Kazuko Tsurumi and Mark Peattie. For those interested in a poignant and thoroughly documented history of the Pacific War by one of postwar Japan's most influential scholars, and by one who also lived through the events he describes, I recommend this book highly.

RICHARD J. SMETHURST
University of Pittsburgh

JAGADISH NARAYAN SARKAR. *History of History-Writing in Medieval India: Contemporary Historians: An Introduction to Medieval Indian Historiography.* Calcutta: Ratna Prakashan. 1977. Pp. xix, 195. $14.00.

In this little book Jagadish Narayan Sarkar has undertaken too large a task: (1) a history of Hindu historiography including nonhistorical and semihistorical works; (2) a history of Muslim historiography as it developed in the Islamic world beyond India (Spain included), covering works in both Arabic and Persian of scores of historians from Ibn Ishaq (d. 767) to Ibn Khaldun (1332–1406); and, finally, the main subject of the book, (3) medieval Indo-Muslim historiography of the period between 700 and 1857 A.D.—and all this "in the background of Western historiography" (p. xv). He does all this, the author says, so as not to treat the history of medieval India in isolation.

The conceptual framework of the book, as evidenced by the elaborate table of contents, is admirable. Hardly any aspect of history that will be of interest to a modern critical historian seems to be missing; present also are all the topics, themes, and categories that will be the concern of a theorist or philosopher of history. Yet the "intelligible and synthetic picture of one phase of intellectual history of medieval India" (p. xv) that the author seeks to give eludes the present viewer, and an admirably conceived work ends up as a catalogue, though a meticulous one, of virtually all historians, memoirists, and chroniclers of all sorts during the period 700 to 1857. The works mentioned and commented upon run into hundreds; hardly any is missed. The treatment in depth of the works of any one historian is just not possible. Al-Baihaqi (ca. 996–1077), who wrote "a comprehensive history of the Ghaznavids in thirty volumes," gets twenty-two lines (p. 78) in which his personality, his associations with the court life, his style of writing, his methodology, and so forth are commented upon and evaluated. Al-Utbi gets even less. Zia-ud-Din Barani of the Delhi Sultanate and Abul Fazl of the Mughal period, however, get respectable attention. In such cases and when the author has undertaken a thematic study of a period, the book becomes interesting, useful, and sometimes provocative. Unfortunately, it is precisely in these areas where this reviewer finds the author letting his readers down unnecessarily. I am quite sure that Sarkar could have expressed his views just as effectively in his own words, but he succumbs to the temptation of most textbook writers in South Asia, who weave long narratives based on choice selection of passages from the books of other, usually Western, historians. It must, however, be said to his credit that he holds firmly to his own views and spiritedly defends Muslim historians of medieval India whenever he feels a Western historian has been injudicious in his assessments of them.

The above points to a fundamental problem of many historical writings on South Asia in the twentieth century. All the works of medieval historians are in Persian; some historical material is in Arabic, too. Over a century ago Sir Henry Elliot of the East India Company, Bengal Civil Service gathered most of the works of medieval Muslim historians and translated selected portions of each with his own comments added here and there. Edited by John Dowson of Sandhurst Staff College after Elliot's death, this became the single most important source for medieval Indian history, *The History of India as Told by Its Own Historians* (eight volumes, 1867–77). Historians who have had access to the orginal works or to their full translations have long realized the shortcomings of these volumes. Many historians still, however, depend on Elliot and Dowson exclusively. Though Sarkar had access to original sources in Persian or their English translations, he, too, has depended on Elliot and Dowson to a great extent. For his chapters on the historiography of Muslims outside India as well, he has used secondary sources in English. The book thus has the advantage of introducing a students who does not read Persian or Arabic to a vast quantity of material relating to the medieval period of Indian history.

The second volume of this book, we are told, will deal with the writings of modern historians of medieval India. One hopes it will be more analytical and interpretive, thus giving us the benefits of Sarkar's insights into the history of historiography of medieval India born out of a life-long attachment to the subject.

MASOOD GHAZNAVI
Rosemont College

KAREN ISAKSEN LEONARD. *Social History of an Indian Caste: The Kayasths of Hyderabad.* Berkeley and Los

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Angeles: University of California Press; for the Center for South and Southeast Asia Studies, University of California, Berkeley. 1978. Pp. xvii, 353. $14.95.

This complex, innovative, and ambitious study reconstructs the history of a prominent Hindu caste group from the mid-eighteenth century to the present in what was the premier princely state of India. The Kayasths of Hyderabad had migrated from the Gangetic plain in north India to the Deccan plateau in south India in search of employment. They are commonly viewed as a cohesive social unit that had a tradition of administrative service and knowledge of Persian and Urdu. Karen Isaksen Leonard demonstrates that Kayasths were marked more by division than coalescence and that they were willing to pursue varied strategies in order to exploit available opportunities.

Any analysis of the Kayasths must be linked to the history of Hyderabad, an anomalous princely state where Muslim rulers initially used Persian and then Urdu and English as the languages of their administration, though the majority of their subjects were Hindu and spoke Telugu or Marathi. Leonard divides the evolution of Hyderabad into four stages: a frontier society in the eighteenth century, when a military aristocracy dominated; the consolidation of a Mughal-style bureaucracy in the early nineteenth century, when recordkeepers were powerful; the transition to a modern, Anglo-Indian administration during the final decades of the nineteenth century, when Western-educated men were sought; and the more recent incorporation of the state into independent India. Leonard's coverage of key developments in each of these stages lacks balance, as there is more information on the earlier periods than on the last one; the reader should not expect a concise history of Hyderabad.

The author's primary focus is on the Kayasths, about whom we receive almost excessive detail at times. In the 1750s the Kayasths began as middle-level military retainers and only achieved the peak of their power as recordkeepers in the Mughlai bureaucracy through their record of loyal service as well as their knowledge of Persian and Urdu. They generally declined when British- and Western-educated Indians were recruited but by the 1930s had begun to recoup their influence by shifts into modern educational institutions and professions. Leonard argues convincingly that for two centuries "families, kin groups, and marriage networks were the characteristic social units. These units and their economic strategies over time have been related to changes in the environment and differential access of families to economic resources" (p. 285). Her conclusion challenges the conventional wisdom, largely based on the work of anthropologists, that caste units based on ritual pollution concepts and scripturally enjoined practices were and are the key elements of Indian social structure. Two examples illustrate Leonard's argument. During the second period of Hyderabadi history, when control of bureaucratic positions was important, Kayasth families, such as the Saksenas, shifted marriage recruitment patterns from inside Hyderabad city to broader areas, emphasized the importance of ties between men through their mothers, wives, and sisters, and permitted bilateral inheritance despite scriptural preference for patrilineal transfer. All of these measures helped to secure the personnel necessary to fill positions in the Mughlai bureaucracy. By the 1930s there were marriages across subcaste lines between Gaurs and Saksenas that linked individuals and families sharing associational and occupational ties. After 1950 the elite Mathurs and Asthanas were marrying across subcaste boundaries and even outside the Kayasth caste. Though there are many reasons for these unorthodox marriages, a primary objective was to broaden the occupational network and to obtain marriage partners of comparable educational and income level.

Leonard's conclusion that class rather than caste or ritual considerations is of primary significance for the Kayasths of Hyderabad will force social historians and social scientists to rethink modes of research and widely held theses. Her study is based on a prodigious number of interviews conducted at three intervals from 1965 to 1976 and on written records in Persian, Urdu, and English that are scattered in private and public archives on three continents. It reveals what can be accomplished by a careful analysis of both oral and written data.

The most debatable part of her thesis is how typical the Kayasths are. They migrated in search of economic opportunity and had a peculiar status in a distinctive political entity. It would have been helpful to have more exploration of the relationships between the Kayasths and local Hindu caste groups. Leonard mentions the construction of various Kayasth temples but does not discuss who went to them. We are told that class cleavages among Kayasths are reflected in their affiliations with the reformist Arya Samaj and the orthodox Sanatan Dharm Sabha, but we learn little about Kayasth interaction with other Hindus in these groups. Kayasth priorities might be related to their status as outsiders and therefore not widely shared. Still, Leonard's extensive data and imaginative analysis stimulate us all to reconsider powerful generalizations. Her pioneering study and that of Frank Conlon on the Chitrapur Saraswat Brahmans in western India are landmarks in the new genre of caste histories.

BARBARA N. RAMUSACK
University of Cincinnati

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GYANENDRA PANDEY. *The Ascendancy of the Congress in Uttar Pradesh, 1926–34: A Study in Imperfect Mobilization.* New York: Oxford University Press. 1978. Pp. xvi, 245. $11.95.

This volume by Gyanendra Pandey stands out amidst the spate of recent studies of processes of political mobilization and politicization. The author's meticulous scholarship, careful weighing of all relevant evidence, and lucid presentation mark the study as high caliber. Pandey's keen grasp of the realities of social structure and organization in north India enables him to pose the significant questions, probe in the appropriate places, and tie into a meaningful whole the results of his thorough investigations.

Pandey rejects the shallow discussions that have marred other studies of politicization. His is an approach in depth, fully aware of the complexities of the structures of social organization in north Indian society and of the dynamics of the process of mobilization. He gives due attention to all important aspects of the social and political process within which politicization has to take place. He knows full well that each society, with its own social structure, forms a matrix of crucial importance for political mobilization. Perhaps even more important, Pandey has an admirable grasp of the interactions, the linkages, and the mechanisms that shape and temper the development of political participation. He is fully at home amidst the intricacies of the agrarian society of north India and of local variations of significance for political mobilization. He is equally well informed about the character and organization of the Congress movement in Uttar Pradesh, about the nature of its leadership at all relevant levels, and about the ways in which the Congress tried to "politicize" segments of the public including rural strata.

Pandey seems to me admirably judicious in his balanced approach, weighing the evidence and giving each actor or factor or institution its due consideration. All of this is based upon careful scholarship and an attention to pertinent detail that is admirable. It calls to mind *Captain Swing* (1969) by George Rudé and Eric Hobsbawm. Pandey explicates with great skill the mingling of popular "protest" and Congress organizational activity. He pays close attention to the mechanisms by which the Congress diffused its ideas and spread its organization in a crucial decade and, *inter alia*, provides a valuable analysis of the causes for the alienation of the bulk of the Muslims from Congress anti-imperialism. At the same time, he correctly assesses the role of the government of India and of the provincial government in exacerbating Hindu-Muslim tensions as a defensive mechanism to weaken the nationalist movement. Most impressive, however, is his treatment of the ways in which the Congress worked to politicize and mobilize the populace, within the self-imposed limitations of its own ideological stance.

Not a book for popular consumption, even though it is very well written and lucid, this book will be warmly welcomed by all serious students of Indian nationalism, of modern South Asian history and politics, and of the complex processes of politicization and political mobilization in non-European nations. I commend it highly.

ROBERT I. CRANE
Syracuse University

JAMES D. HUNT. *Gandhi in London.* Preface by STEPHEN HAY. New Delhi: Promilla; distributed by South Asia Books, Columbia, Mo. 1978. Pp. xi, 264. $18.50.

James D. Hunt has sedulously tracked down every shred of information to be had concerning Mahatma Gandhi's five visits to London. The result is a fine monograph on a secondary but illuminating aspect of Gandhi's life. The story of Gandhi's first stay in London as a modest law student from 1888 to 1891 has already been well told in Chandran Devanesen's *The Making of the Mahatma* (1969). And Gandhi's last visit in 1931 is well documented because by then Gandhi had become a world-renowned celebrity followed everywhere by reporters. The particular strength of Hunt's book is its focus on Gandhi's London visits in the intervening years, in 1906, 1909, and 1914.

Immediately following World War I, Gandhi was to transform Indian nationalism with a new technique of confrontation politics. He evolved that technique in large part in the decade before the war, and Hunt demonstrates that Gandhi's three London visits in this decade contributed importantly to his thinking about the nature of valid political action. In 1909, for example, Gandhi met Indians in London who advocated terrorism, and it was on the ship home that he wrote his most famous tract, *Hind Swaraj*, in an attempt to answer the terrorists of London. Equally provocative to Gandhi's reflections was the movement in England for women's suffrage. Gandhi was clearly impressed, writing, "Today the whole country is laughing at them, and they have only a few people on their side. But undaunted, these women work on steadfast in their cause. They are bound to succeed . . ." (p. 102). Yet Gandhi also censured those women who went "to the extent of breaking Mr. Asquith's windows." Noting that "those who want the franchise are in a minority, whereas the majority of women oppose the demand," Gandhi concluded that "the minority has no option but to suffer for long time. If

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demoralized by suffering, they take extreme measures and resort to violence, they will lose whatever sympathy they have . . ." (p. 139).

Gandhi was again on his way to London in August 1914 when war broke out. Present in London during the first days of World War I, Gandhi was deeply impressed by the nation's willing response to a call for voluntary sacrifice. "There was no panic," Gandhi wrote, "but all were busy helping to the best of their ability" (p. 180).

Gandhi's greatness as a revolutionary leader rested in large measure on his clear-eyed ability to recognize the strengths of his antagonists. On each of his trips to London, Gandhi saw something to admire, something worth appropriating for his own purposes. Hunt has made a valuable contribution to our understanding of Gandhi by documenting fully the fact that Gandhi's ability to oppose British rule in India was enhanced by his astute observation of aspects of British life.

FRANCIS G. HUTCHINS
West Franklin, New Hampshire

JAMES JUPP. *Sri Lanka—Third World Democracy.* (Studies in Commonwealth Politics and History, number 6.) London: Frank Cass and Co.; distributed by Biblio Distribution Center, Totowa, N.J., 1978. Pp. xxi, 423. $29.50.

Ceylon by 1947 "was every bit as modern in terms of political institutions and practices as the great majority of advanced industrial nations" (p. 349), writes James Jupp. What has been happening in that island society in the past three decades is not, therefore, a "transition" from "tradition" to "modernity" in politics. If we use the Weberian model of political modernity we may, indeed, find Sri Lanka today less "modern" than the same country, Ceylon, was in 1947. But Western models break down in Asia and Africa.

Jupp is sensitive to the realities of Sri Lanka's society, in which he firmly plants its politics before trying to explain them. He also knows its modern history thoroughly. A senior lecturer at the University of York, he has been intellectually engaged for over twenty years with all the basic questions of how one of the world's established democracies manages its political life, rather than—thankfully—with trying to fit Sri Lanka into Western academic models.

He has written a successful book, brought up to date through the stunning shift of government in the election of July 1977. Though discursive rather than succinct in literary style, the work covers the expected categories for a full-scale political analysis—recent political history, the structure of the system, parties, public opinion and interest groups, elections, and the policies of the governments holding office. He explains the marvel of how the politically sophisticated people of Sri Lanka have reformed, even revolutionized, their society while adhering to democracy. In Asia, only India approaches the political success evident in its neighbor to the south.

In a capstone final chapter, which deserves extracting for special reading by all who study modern political history, Jupp suggests that in countries such as Sri Lanka the "international economic system sustained by the industrial capitalist democracies" is undermining the political system that free enterprise is supposed to encourage (p. 361). He sends a painful and justified reminder to Americans that during the Vietnam war (and even today) "enormous resources were spent for the military defense of a democracy which did not exist while small sums were grudgingly made available . . . to a democracy which not only survived but enjoyed the active support of most of its people" (p. 360).

CHARLES H. HEIMSATH
American University

WALTER F. VELLA, assisted by DOROTHY B. VELLA. *Chaiyo!: King Vajiravudh and the Development of Thai Nationalism.* Honolulu: University Press of Hawaii. 1978. Pp. xvi, 347. $20.00.

King Vajiravudh of Siam, who reigned from 1910 to 1925, has long been regarded as the founder of modern Thai nationalism. Perhaps more than any other leader, he gave a voice to the Siamese nation and reaffirmed its values, paradoxically in such a manner as to hasten the transformation from the absolute monarchy of old Siam to the constitutional monarchy and military autocracy of modern Thailand.

Walter F. Vella's careful examination of Vajiravudh's reign is long delayed in publication but well worth the wait. In a thorough, systematic fashion he has outlined the main developments that bear on the king's "overall program of nationalism." Having "resisted temptations to digress into an overall history of the reign or an examination of theories of nationalism" (p. ix), he more modestly traces the king's early career, the pageantry of his coronation in 1911 and its deliberate use "to further a program of nation building by swelling his people's awareness of nation" (p. 25), and his creation of the paramilitary Wild Tiger Corps. Vella considers briefly the abortive coup of 1912 and, more extensively, Vajiravudh's defense of the monarchy and resistance to constitutional reform. His chapter entitled "National Survival and Militarism" treats the king's efforts to popularize the military and the idea of dying for one's nation, and the author discusses

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the Siamese role in World War I. A fascinating chapter, "The West as Model," discusses the introduction of surnames, the promotion of team sports, the improvement of the status of women, and Siamese economic nationalism. Later chapters cover Vajiravudh's concept of Siamese nationality, his reinforcing and reemphasizing of the Siamese cultural inheritance, and his consummate use of the mass media.

Vella's material is mostly new to the Western reader, who will appreciate his extensive use of the king's own writings in Thai and English. His account also is based upon extensive archival sources, many contemporary newspapers and periodicals, and primary and secondary sources in both Thai and English.

By choosing not to consider the "overall history of the reign" or theories of nationalism, Vella has left himself open to criticism. This book tells us much of what the king said and did but offers little understanding of why he had the influence that he thought (and Vella asserts) he did. Because Vella says so little about the political, social, economic, and cultural changes of the reign, the reader cannot easily relate Vajiravudh's life and works to his society. Vella might briefly have mentioned the dissertations of Stephen L. W. Greene (London, 1971), Tej Bunnag (Oxford, 1968), and Peter B. Oblas (Michigan, 1974) to flesh out the political, fiscal, administrative, and diplomatic history of the period. Similarly, by failing satisfactorily to define "nationalism" or its relationship to "modernization," Vella provides the reader only with a vague impression that Vajiravudh's thoughts and activities somehow encompass Siamese "nationalism." Vella's analytical escapism relieves him of the confrontation with the paradox of Vajiravudh defending the absolute monarchy and resisting moderate constitutional reforms in patriarchal terms while setting in motion the forces that were to topple the monarchy only seven years after his death.

DAVID K. WYATT
Cornell University

GEORGE RUDÉ. *Protest and Punishment: The Story of the Social and Political Protesters Transported to Australia, 1788–1868.* Oxford: Clarendon Press. 1978. Pp. x, 270. $22.00.

This work of George Rudé has a narrow purpose: to identify as clearly as possible those who were convicted of offenses of a social or political nature in Britain or Ireland, and to follow them into penal transportation to Australia and trace their fortunes there. It enters only peripherally into the stream of Australian historical study, which has been concerned with the issue of whether those transported were the "village Hampdens" of the Hammonds or the larcenous riffraff discerned in studies of convictism by A. G. L. Shaw and Lloyd Robson.

Rudé admits fully the difficulty in making the necessary distinction between the social or industrial protester and the general run of criminal. Despite a rigorous effort at such differentiation, he concedes that the line is fuzzy and that there were many convicted for offenses that could only marginally be regarded as social protest. Arson, cattle maiming, smuggling, poaching, and the sending of threatening letters are all offenses in which the degree of social protest is hard to define. Much would depend, as Rudé states, on the circumstances surrounding the act, and the traditions and mores of the local community. A complicating additional factor was that some transportees committed crimes because they felt that they might better their condition by being carried to Australia. Rudé arrives at what he regards as a plausible, though not fully accurate, count of some 3,600 prisoners who were dispatched to the Australian colonies for crimes of social or political protest; this was about 3 percent of the 163,000 prisoners sent there.

The information concerning the members of this group after their arrival in the Australian colonies is sparse. Rudé's comparison of the protesters with the general run of convicts is largely confined to matters of age, literacy, marital status, and general criminal record. Even concerning Rudé's old friends, the participants in the Captain Swing outbreak of 1830, of whom some 480 went to Australia under sentence, little is known after they received pardons and became free men. They disappear into the general stock of the Australian laboring class, and this is true of most of the protesters.

If there were village Hampdens, Rudé would have posterity do them justice. But even with the best of intentions, he is not able to give them much more than names; they remain faceless characters. Those whom he is able to trace seem to have had little impact on the development of social egalitarianism or political democracy in Australia, despite their background of protest in the United Kingdom. Rudé's work tends by inference to support the judgment of previous scholars that few if any of the protesters carried political or social principles into action once they were free men in Australia. Even among the more rebellious Irish, such principles seem to have vanished within a few years after reaching Australia. Apparently the penal experience and the wider opportunities and greater social equality that many found in Australia doused the fires of rebellion that had brought them there.

DONALD C. GORDON
University of Maryland, College Park

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T. B. MILLAR. *Australia in Peace and War: External Relations, 1788–1977.* New York: St. Martin's Press. 1978. Pp. xxiii, 578. $25.00.

This is a big, wide-ranging book by an established specialist about Australian responses to its external environment. Considerable historical dimension is provided for the author's more recent, essentially post-World War II emphasis. In this sense T. B. Millar deals not only with the period that has had a conventionally understood foreign policy, but with antecedents, reaching back to prefederation, colonial times.

Millar states early on that he has not set out to write a book cast within some formal, analytical model or to transfer consciously the lessons of Australia's experience to other settings. The product is an informed and valuable piece of writing, by its nature a synthesis. It is not done from the vantage point of a "revisionist" or some other, discernible intellectual perspective. Millar, by and large, stands personally aside from the controversies he discusses, though there are occasional lapses into language suggesting that virtue and principle in international politics may be their own reward.

Readers of a book of this scope and ambition will inevitably find some aspects of emphasis and organization not to their full liking. For my part, I feel that the effort to build a multitude of functionally and regionally titled chapters into broader historical sections has produced an overly wavy, sometimes thin, line of continuity of presentation and argument. Millar says that his book is basically about present Australian foreign policy and is not simply recent history. But his treatment of the Labor government of 1972–75 and of the succeeding Fraser government is sketchy, and more explicit underscoring of the continuity/change line of preceding outlooks and policies might have helped. Guiding policy rationales are, however, explicated more plainly in these closing chapters than in the material preceding them. Millar introduces an early chapter on foreign policy decision-making processes that is useful in and of itself but whose content is only very slightly drawn upon in the body of the book.

Nevertheless, Millar's book stands as a very important contribution, free of distracting oversimplification or argumentativeness, especially for those who desire overview. The author knows his subject, and conversant readers, as well as those to whom Australia is unfamiliar, will find it convenient to have this kind of knowledgeable, single-volume survey on their shelves.

There are of course the large themes, such as Australia's efforts to cope with its unusual Asian-Pacific context, the opportunities as well as constraints imposed on Australia by reason of its modest resources of influence, and the uneven movement from dependency to partnership and initiative. The book covers many not too distant but instructive episodes that are part of international as well as Australian diplomatic history, for instance postwar U.S.-Australian differences relating to the security of Pacific islands, the treatment of Japan, and the role of the Australian troop commitment in what was then Malaya. One of the special touches Millar brings to his book is his interest in the dominant personalities of the period. Policy and its presentation are made more lively with the sharpened focus on the roles of individual foreign and prime ministers.

HENRY S. ALBINSKI
Pennsylvania State University

S. G. FOSTER. *Colonial Improver: Edward Deas Thomson, 1800-1879.* Carlton, Victoria: Melbourne University Press; distributed by International Scholarly Book Service, Forest Grove, Oreg. 1978. Pp. xv, 209. $19.95.

In this all too short biography, Edward Deas Thomson has received his just due as one of Australia's ablest administrators. He has been overlooked, in the main, by historians, and S. G. Foster suggests a possible reason for this neglect. Thomson is "everybody's picture of a dull dog: as a young man, a calculating and self-righteous prig; in his prime, a staid pillar of the establishment; in old age, an irritable opponent of change. Nevertheless, he was a key figure during a formative period in the politics of New South Wales" (p. xiv). Born in Edinburgh, he was educated at Harrow and later studied for two years at a college in France. He traveled in the United States (1826–27) and in 1829 arrived in Sydney as clerk of the New South Wales Legislative and Executive Councils. His intelligence, competence, and industry won him the post of colonial secretary in 1837, and he served under three governors: Sir Richard Bourke, Sir George Gipps, and Sir Charles FitzRoy. In 1856 the position, second only to that of the governor, ended with the coming of responsible government.

Although he was proud to identify himself with the colony's advancement, he was under great pressure after representative government came in 1843 since he had to serve two masters: the governor and the elected Legislative Council. Because he was shrewd, tactful, and discrete, he was a successful administrator. Foster has written a detailed portrait of Thomson by using with care the overlooked Thomson papers, the Bourke papers, and the very large corpus of Colonial Secretary papers—all to be found in the Mitchell Library, Sydney. Thomson married one of Governor Bourke's daughters, and she corresponded at length with her father who had

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retired to Ireland (1837–55). Her letters reported many of Thomson's political problems. Thomson's letters to his own father do little to change the current view of Bourke and FitzRoy; but his picture of Gipps is much darker than the 1966 entry in the *Australian Dictionary of Biography*. To Thomson, Gipps was stubborn, hesitant and cautious, abrupt and impetuous, indiscreet and disrespectful, and, while quick and clever, "he was by no means accurate: many papers required to be returned to him for decisions upon matters which he had entirely overlooked in his minutes" (p. 55). Thomson's judgment must be respected because he was unusually cautious and objective and also because, despite his private reservations, he was a warm and intimate friend of Gipps.

Foster has concentrated on Thomson's political career. He was a "lucid, logical and persuasive" debater, and, by urging moderation, he was an influential force in the Executive Council. From 1855 to his death he showed his close interest in the development of railways, in the growth of the University of Sydney (which he served briefly as vice chancellor and chancellor), and in the expansion of a free trade policy.

Foster has compressed his findings into 154 pages, excluding the excellent footnotes and bibliography. He might have used the Colonial Secretary papers to illuminate Thomson's views on such subjects as aboriginal policies, the growth of free education from 1830 to 1850, the many exploration parties, and the efforts to improve the care and rehabilitation of convicts, such as Alexander Maconochie's "mark system" at Norfolk Island from 1840 to 1844.

SAMUEL CLYDE MCCULLOCH
University of California, Irvine

CHARLOTTE CARR-GREGG. *Japanese Prisoners of War in Revolt: The Outbreaks at Featherston and Cowra during World War II.* New York: St. Martin's Press. 1978. Pp. xvi, 225. $17.95.

Even experts on the Pacific War know little about large-scale uprisings by the relatively few Japanese who fell into Allied hands. Actually there were two serious revolts: one at Featherston, New Zealand (February 1943) when guards killed forty-eight Japanese and wounded sixty-three; the second at Cowra, Australia (August 1944) when two hundred thirty-four Japanese were slain, one hundred eight wounded. Charlotte Carr-Gregg, a social anthropologist at the University of Victoria in New Zealand, was intrigued by a routine postwar visit to Cowra town and by Hugh Clarke's account of the "mass suicide attempt" in *Break-Out!* (1965). After learning of the Featherston mutiny, Carr-Gregg undertook to investigate and compare the incidents, seeking "a characteristic, peculiarly Japanese pattern to account for the behaviour of the prisoners."

Interposing initial difficulties, until convinced of her *bona fides*, were the Australian Archives Office and War Memorial Museum, New Zealand National Archives, and Red Cross International Committee. While playing down any cover-up, the author wonders whether "official reluctance to release the documentation ... may reflect an almost universally shared sense of guilt about the frequently unnecessary sufferings endured by prisoners of war, irrespective of their nationality" (p. 7).

From the archival materials, English-language literature, and translations made for her from published Japanese works and correspondence with a few Japanese survivors, Carr-Gregg has examined the prisoner status in Western and Japanese milieux, the Featherston and Cowra episodes, cultural continuity in society, Japanese personality and value orientation, and external influences. The weakest, least valuable part is the thin but lengthy survey of Japanese history and culture from mythological origins. The secondary sources are highly respectable (Sansom, Reischauer, Storry), and the author delves astutely into translated excerpts from *Hagakure*, the eighteenth-century Book of the Warrior, and *Kokutai no Hongi*, the latter-day Fundamentals of National Polity. Nevertheless, no new ground is broken, and confidence is shaken by numerous misspellings of Japanese words, wrong dates, and gaps in Japanese military taboos and in *seishin kyoiku*, spiritual training. More importantly, one wonders about the real relevance of the Amaterasu and Susano-o legends to lowly, isolated Japanese prisoners.

Much more valuable is Carr-Gregg's explication of Japanese personality structure and values encompassing role dispossession, self-annihilation, self-dramatization, compulsiveness, and collaterality. The final hypotheses are eminently defensible: that the Cowra rising in particular combined a quest for death, planned escape, and spontaneous reaction. Comparisons to other wartime experience, ranging from "Hogan's Heroes" in Europe to Andersonville in the Civil War, would have profited from examination of the Koje-do and Pongam-do riots of 1952 during the Korean War.

The postscript material is interesting; for example, the commentary by one ex-prisoner dreading the returnees' lot: "When we came back to Japan, all Japanese were just like prisoners because of their defeat in the war. So all of them welcomed me joyfully" (p. 203). Such tantalizing nuggets suggest the untapped potential of oral history conducted before the last survivors vanish. Although Carr-Gregg's study, with its generally helpful illustrations, exudes sympathy, fairness, and conscientiousness, an

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uneasy feeling lingers that, despite excellent clinical diagnosis, we still comprehend little concerning the innermost subjective recesses of the disoriented Japanese prisoners incarcerated at Featherston and Cowra.

ALVIN D. COOX
San Diego State University

NIEL GUNSON. *Messengers of Grace: Evangelical Missionaries in the South Seas, 1797–1860.* New York: Oxford University Press. 1978. Pp. x, 437. $39.50.

To the English evangelical missionaries of the late eighteenth and early nineteenth centuries, salvation was the business of life. Drawn from Britain's lower classes, these mechanics—or manually skilled men of God—sought to save their own souls by taking Christianity to the islands of the South Pacific. Their efforts had the markings of a class struggle; the "poor heathen" replaced the "miserable poor" in the evangelicals' scheme of things. Success was measured neither by the number of converts nor by the quality of work. The culmination of seventy years of intensive missionary activity in the South Pacific lay in the recognition of these men as proper ambassadors of Victorian mores.

Multifarious problems confronted the missionaries in the field. Distance, communication failures, illness, isolation, family problems, language barriers, island politics, rowdy seamen, resident traders, and itinerant British officials all hampered their efforts. The most serious obstacle, however, was the missionaries' own failure to understand and appreciate the cultures in which they worked. "The evangelical missionary had little doubt that Satan, adversary of God and man, reigned as absolute sovereign over the South Sea islanders" (p. 195).

Unlike their Roman Catholic counterparts, who stressed accommodation to local cultural forms, the English Calvinists and the Wesleyans shunned compromise. These men viewed the eradication of the existing social order as necessary for conversion. The evangelical gospel meant the spread of Western ways. The missionaries held firmly to the tenet that "the entire social system of the Anglo-Saxons would make the islanders happier. They did not regard it as a problem of adjustment but merely as a simple change from ignorance to the 'civilized way' " (p. 272). Their emphasis on a uniform code of Christian conduct contrasted sharply with the different patterns of behavior exhibited by other groups of Westerners in the islands, who sorely tested the missionaries' credibility.

The adoption of Christianity did offer the island people psychological shelter from disease, death, and the tidal waves of cultural flux and confusion brought by foreigners, in short, a way of coping. Christianity also presented political and economic advantages to some. In the end, though, it was more the force of the British Navy than the power of the Word that caused Christianity and "civilization" to take hold in the islands of the South and Southwest Pacific.

Surprisingly, the author seems to share some of the ethnocentricity of his subjects. Questionable is his assertion that the missionaries possessed "superior skills." Disconcerting is Gunson's acceptance of the bias that the Asian missions required better trained and more intellectual men than the Pacific missions.

The book gives little attention to the story of the American Board of Commissioners for Foreign Missions, whose social origins and aspirations differed considerably from their English brothers'. Still, in getting beyond most of the missionary propaganda of the era, the book succeeds admirably in revealing the mentality of the British South Seas missionaries. Of special note are the extensive calendar of manuscript sources and the selected bibliography of printed materials. Gunson's work invites a search for the still elusive indigenous accounts so necessary for a more complete understanding of the missionary period in this fascinating part of the world.

DAVID L. HANLON
Community College of Micronesia

UNITED STATES

JAMES H. KETTNER *The Development of American Citizenship, 1608–1870.* Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1978. Pp. xi, 391. $20.00.

Running through the early years of American history—surfacing at crises in 1776, in 1798, throughout the issues of right of search before and during the War of 1812, in the Dred Scott case, and notably in the years 1861–65—was the question: what was it to be an American citizen? How many years of residence were required, and was residence in itself enough? The problem did not originate in 1776. English law stressed the personal nature of the relationship between king and subject, the assumption of a pledge of allegiance even when it could not be made in person or by ownership of a fee, and the hierarchy of ranks that arose from an old and fixed social system. There was no clear sense of nationality, and "Frenchmen in the medieval period could not hold English lands not because they were aliens but because they were at war with the English king. Not until the seventeenth century did the notion of residence rather than allegiance begin to determine status, and not until the opinion of Sir Edward

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Coke in *Calvin's Case* in 1608, after the union of the English and Scottish crowns, was a theory of allegiance and subjectship fully articulated. This was still a subject-king relationship, personal and deemed to be perpetual. Even more emotionally than juridically, it grew rather than declined: in 1899 even more than in 1776, in 1914 and in 1939, the concept of King and Country were all but inseparable.

To American even as colonists this was not enough. The need to attract settlers required generous naturalization policies that would give aliens the same rights as Englishmen. The struggle for independence made the seventeenth-century theories of the social compact persuasive—allegiance should be the result not of birth or status but of consent, an act of will. Clearly, from one point of view, the Loyalists were true to an older and personal allegiance. Yet from another they were enemies of the new state. It was easier for individual states to deal with them than for new principles of citizenship to emerge. The war against the "ministerial" or "parliamentary" army was being fought for a year before the break took place with the king. It was, however, in facing such issues that Americans developed the idea of volitional allegiance.

James H. Kettner explores this complex but fascinating subject with authority and dexterity. His book, which has already won the 1975 Jamestown Prize, shows a command of English as well as of American constitutional law and is firmly rooted in statutes and cases, admirably indexed. Kettner makes clear how deeply rooted in English history and seventeenth-century English case law are the concepts of "natural-born citizen," "naturalization," and "birthright citizenship." The change, essential because of independence, made it possible for the United States to welcome the immigrant and legitimize his presence. It also destroyed dependence on a personal ruler; the new society was—in theory at least—one of free men freely choosing to be Americans. It left to be resolved the form of the community of allegiance and the location of ultimate sovereign power; "We the People" might be one sovereign community or many. In theory if the individual could freely join, he could freely leave, but could a slave? And those who were already there without choice and were born without choice—blacks and natives and women—had a different view of the community.

Kettner sees the judicial problems as largely resolved by 1870. The implications were to require a stormy century to resolve themselves. He admits that the social and cultural forces serve mainly as background, and he does not explore the contrasting views of citizenship, and of the nature of patriotism and nationalism, of our two countries. Impeccable and authoritative as is the primary source material, a brief bibliographical note would have been valuable. This is a splendid and original piece of research, however, elegantly written and happily free from technical legal language.

ESMOND WRIGHT
University of London

SARAH SLAVIN SCHRAMM. *Plow Women Rather Than Reapers: An Intellectual History of Feminism in the United States.* Metuchen, N.J.: Scarecrow Press. 1979. Pp. ix, 441. $17.50.

The challenge of stating precisely what they are about has long perplexed feminists and their critics. In *Plow Women Rather Than Reapers,* Sarah Slavin Schramm has set herself the ambitious task of identifying feminism's intellectual framework, chiefly in the United States, by exploring the relevant thought from the seventeenth century to 1960.

Schramm, an avowed feminist, focuses on familiar themes: the paradox between women's strivings toward self-actualization and the frequent conflicts imposed by marriage and family; education for women; the "inapposite ideologies" (p. 5) of liberty and sexual equality; and, finally, the debate over appropriate sex roles. Here the author identifies two streams of thought, categorizing feminists on the basis of their advocacy of complementary or congruent roles for women and men.

From the outset, the author's attempt to incorporate the views of nearly 140 prefeminist and feminist thinkers leads to problems. In "Feminism and Family," for example, such diverse writers as Mary Wollstonecraft, Sinclair Lewis, Margaret Mead, Henry James, Olive Schreiner, Ellen Key, Charlotte Perkins Gilman, Suzanne LaFollette, Jane Addams, and Doris Lessing—to mention just a few—are paraded across the pages so rapidly that comparative analysis breaks down. By rejecting a systematic examination of individual feminist thinkers, Schramm sacrifices continuity and cohesion. She attempts to compensate for this by utilizing an unusual approach to intellectual history, offering the reader an overview of feminism's components through fantasized dialogues among prefeminist and feminist thinkers meeting "apart from time and space" (p. 19).

This work would have benefited from more skillful editing. Awkward, obtuse, and convoluted sentences abound. Given the book's range, confusion results from the author's preference for casting her writing in the present tense. The index is poorly done; there are more than fifty separate pages listed for Mary Wollstonecraft, but no subentries are provided. The annotated bibliography is comprehensive and innovative, with separate categories of empirical and normative complementary and con-

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gruent writers. Students will find the biographical listing useful.

Despite defects, Schramm's book does demonstrate the persistence of feminist thought. Nor can one argue with the conclusion that feminism has consisted of "on-going generalizations" that "never reach[ed] the status of full-blown ideology" (p. 342). Schramm's own feminism is pragmatic, offering a strategy for change. Although not all will agree with her separatism, she is at her best when discussing the "idea power" and "promise" (pp. 347, 351) of women's studies programs as vehicles through which a feminist ideology may yet emerge.

JANE B. DONEGAN
Onondaga Community College

REDDING S. SUGG, JR. *Motherteacher: The Feminization of American Education.* Charlottesville: University Press of Virginia. 1978. Pp. xi, 282. $14.95.

Contemporary education is "soft," according to Redding S. Sugg, Jr. Elementary and secondary schools, even colleges, have sacrificed instruction in basic skills and mastery of content for education centered on student interest. Over the classroom hovers "Motherteacher," a person once exclusively female but today increasingly male as that sex yields its authority and discipline. Permissive education inadequately prepares students for the hard choices demanded by nuclear power, overpopulation, and diminishing natural resources.

Sugg traces the "soft line" in education from the early republic, when the American people rejected proposals for a national university, through such nineteenth-century educators as Catharine Beecher, Horace Mann, Edward A. Sheldon, and Francis W. Parker to G. Stanley Hall, John Dewey, William H. Kilpatrick, and Harold Rugg. By popularizing the new view of childhood as a state of innocence, ministers and educators provided a rationale for the feminization of teaching. Young pupils, implicitly male, needed woman's special nurturing talents. Women teachers, Mann believed, were motivated by "the doctrine of pedagogic love" (p. 75). They had gentle manners, pure morals, and were "less intent and scheming [than men] for future honors and emoluments" (p. 74). Because teaching offered them higher social position than other job alternatives—domestic service, practical nursing, and factory work—Mann's normalites accepted one-fourth to one-half the pay men received. As women entered teaching (72 percent of teachers were female by 1900), its professional standing declined. To maintain status, men became a better-paid "male priesthood of administrators and bureaucrats" (p. 85).

Motherteacher evolved full-blown from a distortion of Dewey's concern for "psychologizing" subject matter. She encouraged correct social growth by "muddling along" with her pupils in deciding what was taught. By the 1960s, Motherteacher was "no-teacher." To find guidelines for the rehabilitation of schooling, Sugg urges a re-examination of educational historiography. If countercurrents in the thinking of William T. Harris, Lester Frank Ward, and Dewey were adopted, schools could develop "intellectural vertebrae" and function as an integral part of an educational hierarchy. Concurrently, both sexes should be balanced at all levels and positions.

While Sugg discusses masculine reactions to femi nization—the masters of the Boston public schools in 1844–46 and the Male High School Teachers Association of New York City in 1904—he mentions few instances in which women acted decisively. He does not explain why four women challenged the Massachusetts Supreme Court by running in 1873 for the Boston School Committee; they were permitted to serve only after the legislature removed their disqualification. Nor does he explore women's dissatisfaction with lower salaries beyond commenting that after the consolidation of the New York City school system in 1898 "the women teachers had vociferously demanded equal pay with men for equal work" (p. 118). This was no isolated protest. Susan B. Anthony, Margaret Haley, Catherine Goggin, Kate Hogan, Grace Strachan, and Ella Flagg Young showed that some women educators were feminists dedicated to improving the professional status of women teachers.

Sugg's analysis of educational historiography would be helpful to interested general readers. Specialists may find his treatment of the complex problems of feminization less satisfactory.

MARCIA G. SYNNOTT
University of South Carolina

LOUIS FILLER. *Vanguards and Followers: Youth in the American Tradition.* Chicago: Nelson-Hall. 1978. Pp. viii, 252. Cloth $11.95, paper $6.95.

If the sixties did nothing else, they alerted Americans to the insistent presence of youth and sent many historians to investigate previous "youth movements" and to analyze the role of youth in history. Where the search resulted from a sincere need to understand, the awareness of youth has led to a fuller picture of the past and to a sharpening of the analytic tools by which we learn how societies and cultures operate and evolve. But the sixties also had a less salutary effect in the alarm and fear the decade provoked. Louis Filler's book is neither about the role of "youth" in the American tradition, as the subtitle suggests, nor a history. It is a personal testament to his anger at the youth of the sixties.

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It would be generous to describe this book as methodologically flawed: it has no methodology. In the "historical" chapters, the reader is invited to see "youth" as everything new and challenging in American culture of the eighteenth and nineteenth centuries; then to see "youth" as any and all of the bohemian-literary rebels of the early twentieth century; finally as the alienated radicals of the thirties. In these chapters, age is irrelevant, and Filler simply skims the unorthodox cultural expressions of the American past by amiably gossiping about the eccentricities (largely alcoholic and sexual) of various individuals. He reduces culture to personality, indiscriminately including a varied assortment of characters like Walt Whitman, Lola Montez, Randolph Bourne, and Gertrude Stein. All are equally "youth" and somehow are intended to represent the behavior and aspirations of American youth generally (in the ghettos, in the colleges, in the sweatshops?). In the process, Filler does more than fail to define who or what youth was, he trivializes the lives and thought of those whom he chooses to include, portrayed here as merely cheerful and silly or morose and self-indulgent.

Were youth in the past his central concern, this could be dismissed as genially irrelevant. In fact, however, these chapters serve as an excuse for Filler to give his version of the sixties—a grab bag of irresponsible, mindless behaviors, without motiviation and without context. As he approaches the sixties, Filler's account becomes heavily detailed and his earlier tone of distant disdain for trivial youth turns passionate and sarcastic. With evident disgust, he evokes a picture of youth writhing in filth in the gutters and hovels of San Francisco and New York, drugged, violent, and ugly. Filler describes the sixties as a long train of meaningless, puerile acts of black youths, antiwar youth, campus youth, all without cause, without even a voice (except for inarticulate obscenity), and their sycophants in the faculty, politics, and the media, culminating in the unspeakable crimes of Charles Manson. Along the way, there are broken lives and broken bodies, the destruction of values and politics, a disregard for decency and patriotism. The sixties clearly hurt Filler profoundly and he has voiced his pain in this book.

Each person, even the historian, has a right to his own view of the sixties, and each can draw a personal moral. But the historian also has an obligation to rational inquiry. It would be a great disservice to serious historians, many of whom first began to explore the subject of youth in the context of the events of the sixties, to include this book within the growing literature on youth in history.

PAULA S. FASS
University of California, Berkeley

PETER WILD. *Pioneer Conservationists of Western America.* Missoula, Mont.: Mountain Press. 1979. Pp. xxv, 246. $12.95.

Poet Peter Wild uses the word "pioneer" to describe those who viewed the land in new ways and attempted to provide more enlightened solutions to environmental problems. To his credit, Wild discusses lesser known and contemporary persons as well as acknowledged past leaders of conservation. His list includes men and women of letters (Mary Hunter Austin, Bernard DeVoto, Joseph Wood Krutch, Edward Abbey), men in government office (John Wesley Powell, Gifford Pinchot, Stephen Mather, Stewart Udall), and those whose contributions or careers cut across diverse fields (John Muir, Enos Mills, Olaus Murie, Aldo Leopold, William O. Douglas, Garrett Hardin, David Brower).

Wild is more concerned with the fifteen conservationists he describes and how their personalities affected their contributions than he is in presenting an overview of environmental history or in placing the conservationsists in the context of their times. For example, he makes no connection between the Progressive era and Pinchot's conservation philosophy and program, and he omits any mention of the 1908 Governor's Conference, perhaps the high point of the first conservation movement. Wild is more interested in literary protest against destructive land use than with practical efforts to cope with land use problems. Although he concludes that our "addiction" to technology is the crux of our environmental dilemma, he offers little to explain why technological development and economic concentration followed the course they did.

On occasion Wild falls into factual errors and distortions. It is not true that Sequoia and Yosemite National Parks were set aside "only when a local populace grew heady on visions of tourist dollars and was able to outshout the local miners and lumbermen" (p. 60). It is not accurate to state that at the time of the appointment of Brower as executive secretary of the Sierra Club that the Club "had not won a single major environmental battle in the thirty-eight years since Muir's death" (p. 154). Wild's criticism of almost all congressional land legislation in the nineteenth century because it allegedly benefited the wealthy fails to consider that land speculation was a way of life for rich and poor alike and that much of the land, as Paul Gates has illustrated, reached the hands of the yeoman farmer.

Because Wild makes no pretense of presenting an original historical interpretation, restricts his research, and provides no documentation, his book will be of limited value to students of environmental history and the American West. *Pioneer Conservationists*, however, is well-suited for its intended

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audience, a general public interested in the personalities and contributions of leading American conservationists.

DOUGLAS H. STRONG
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A. THEODORE BROWN and LYLE W. DORSETT. *K. C.: A History of Kansas City, Missouri.* (Western Urban History Series, number 2). Boulder, Colo.: Pruett Publishing. 1978. Pp. 303. $13.50.

A. Theodore Brown and Lyle W. Dorsett, each of whom has written scholarly books on Kansas City, turn here to a brief narrative urban "biography" for the general reader. Combining material from the secondary literature with some new research, the authors tell the story of Kansas City from its frontier origins amid slavery troubles to its emergence as an aggressive metropolis that claims to be "one of the few liveable cities left!" Along the way they discuss the promotion of railroad connections by an effective, tightly knit "little power elite" (p. 16) during the Civil War era, the expansion of cattle, railroad, and real estate activities, the fragmentation of leadership and the divergence of business and politics as the city diversified, the proliferation of civic and cultural organizations, the growth of public services, the rise of municipal reform and city beautification movements during the early twentieth century, the spread of new residential districts, the ascendance of Thomas J. Pendergast's machine (which in the 1920s and 1930s built a modicum of community coordination through its brokering of diverse interests and its manipulation of public works and jobs), the development of a city-planning emphasis after the downfall of Pendergast in 1939, and, finally, after 1963, the emergence of a civic-minded, business-oriented, unified "power elite" that gave the city a new "sense of community spirit and direction" (p. 264) and brought everything "up to date" (p. 253).

The treatment is a mixture of once-over-lightly description and loosely woven narrative. The descriptions sketch population groups, neighborhoods, and physical layout at certain intervals but provide little analysis or sense of the dynamics of growth and expansion. The absence of useful outline maps is a distinct drawback. The narrative deals predominately with the careers, projects, and achievements of prominent business and government leaders. In general, the narrative proceeds chronologically, but the chapters are not sharply focused either thematically or according to time period. Readers with a particular interest in Kansas City's past will find here an engaging synthesis with considerable fascinating detail and abundant photographs, but scholars will discern slight substantive or conceptual contribution.

The authors do attempt to characterize the civic leadership of successive periods as either unified under a "power elite" or fragmented into divergent interests. But they use the term "power elite" loosely, without defining it and without specifying a methodology for identifying elites. Neither they nor the secondary studies on which they rely appear to have conducted any systematic comparative evaluation of the impact of the civic policies of the successive leadership groups upon the various social and economic elements of Kansas City's population. Nonetheless, the authors usually frown on fragmented leadership and smile on "power elite" leadership, implying that united business elites have been more effective in nurturing general civic welfare. Thus the book offers a cordial affirmation of the view that the greatest progress has occurred when the city has united behind the initiatives of its most prominent business leaders.

CARL V. HARRIS
University of California, Santa Barbara

DAVID DELEON. *The American as Anarchist: Reflections on Indigenous Radicalism.* Baltimore: Johns Hopkins University Press. 1978. Pp. xiii, 242. $14.00.

American radicalism, argues David DeLeon in this deeply flawed book, differs fundamentally from radicalism in other countries. Elsewhere, says DeLeon, radical movements have been overwhelmingly statist and authoritarian; in America, by contrast, a profound antagonism towards centralized authority has been the hallmark of our radical tradition. From the Puritans of colonial New England to the New Left of the 1960s and 1970s, DeLeon traces a continuous heritage of dissent that is strongly anti-institutional and anarchistic. He tells us to look for the spirit of American radicalism "not in Lenin but in Debs, not in the USSR, but in the IWW, not in Chinese peasants, but in the Populists" (p. 4).

To buttress his thesis, DeLeon examines the three main sources—Protestantism, capitalism, and the peculiarities of American geography—from which the anarchistic character of American dissent is said to derive. Following this, he ranges quickly over the entire spectrum of American radicalism, from the far left to the far right, then surveys the radical movements of the 1960s and 1970s, linking them with earlier schools of dissent.

The results, on the whole, are disappointing, for DeLeon has tried to do too much in too short a space. There is too much facile generalization, too little depth of analysis, and too little attention to

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detail. What DeLeon has written is not a solid scholarly monograph but a loosely reasoned interpretive essay that is marred by oversimplification and distortion. For example, the dichotomy between native and foreign radicalism is too sharply drawn. American radicalism has always been shaped by a combination of indigenous and European influences. Thus Benjamin Tucker, America's leading individualist anarchist, was indebted not only to Josiah Warren, Lysander Spooner, and Stephen Pearl Andrews but also to Max Stirner, Pierre-Joseph Proudhon, and Mikhail Bakunin, whose doctrines he adapted to suit American conditions. By the same token, European radicals have been influenced by an array of American writers, among them Jefferson, Emerson, and Thoreau. Furthermore, a deep distrust of the state has by no means been peculiar to America, as the examples of Italy, Spain, and Russia bear witness.

The weaknesses of DeLeon's analysis are compounded by the errors of fact into which he frequently lapses. To cite a few examples: Alexander Berkman's magazine *The Blast* began publication in 1916, not 1915 (p. 94). Voltairine de Cleyre was not an "Anarcho-Communist" (p. 94), a label she was at pains to disavow. Emma Goldman came to America at the age of sixteen, not eighteen (p. 94). It was not William Dean Howells but Walter Hines Page who serialized Kropotkin's *Memoirs of a Revolutionist* in *The Atlantic Monthly* (p. 96). *The Road to Freedom*, the principal anarchist journal of the 1920s, did not "live in the past" (p. 99) but contained many valuable articles on the burning issues of the day, from the consolidation of the Bolshevik dictatorship to the martyrdom of Sacco and Vanzetti. Henry Bool, the Ithaca, New York, furniture dealer who supported Tucker's publishing ventures, was an Englishman by birth, not a native American (p. 125). Among the many misspelled names in the book are those of Clarence Lee Swartz (not Schwartz), Guy Aldred (not Alfred), Carlo Cafiero, Ettore Zoccoli, Valerian Zorin, George Meany, Eric Hobsbawm, and Agnes Inglis.

The forty-page bibliography, which is riddled with errors and ranges indiscriminately over the entire field of anarchism, European as well as American, is much too long for a book of this scope. Moreover, it lacks any principle of selectivity, listing articles from *Newsweek* and *Playboy* along with works of fundamental scholarly value; and Wendy Snyder's *Haymarket* (1971), which DeLeon includes under the Haymarket affair of 1886–87, deals with the Haymarket district of Boston, not with the famous Chicago case. At the same time, the bibliography is incomplete. It omits, for example, the single most important repository of anarchist materials in the world, the International Institute of Social History in Amsterdam, which contains a wealth of documentation on American anarchism that DeLeon does not appear to have consulted.

PAUL AVRICH
Queens College,
City University of New York

I. A. NEWBY. *The South: A History.* New York: Holt, Rinehart and Winston. 1978. Pp. xvi, 559. $14.95.

In about five hundred pages of text this work presents an "overview" of southern history designed to introduce students "to some of the issues that illuminate its study" (p. xiii). I. A. Newby begins with a brief discussion of the fundamental problems of studying the South and finding a central theme in its history, after which he launches his survey with a consideration of the colonial South. The book is divided into six main parts: The Developing South, 1607–1820; The Old South, 1800–1861; The Fighting South, 1846–1877; The New South, 1870–1917; The Changing South, 1917–1945; and The Present South, Since 1945. The coverage is comprehensive and balanced, although the last section is not as well developed as the other five. Newby is more concerned with ideas and forces than he is with the reconstruction of events and the treatment of regional institutions. Thus his volume differs in approach and emphasis from that of the leading textbook in the field, Francis B. Simkins and Charles P. Roland's *History of the South* (4th ed., 1972).

Newby, a close student of southern history and the author of several studies of Afro-American history and anti-Negro thought, distinguishes between *sectional* and *regional* history—between the South as an idea or distinct entity and the South as a geographical area. Although he does not altogether neglect the South in the latter sense, his focus is the distinctive and peculiarly southern facets of the section's past. He is attentive to the origins of sectional self-awareness in the South, to the components of the southern identity, to the clash between change and continuity in southern history, and to the increasing Americanization of the region in the twentieth century. The distinguishing aspects of the southern experience, according to Newby, are "the obsessive concern with racial matters, the defeat and poverty and frustration that came after the Civil War, the pervasive sense of grievance, [and] the distinctive history of black southerners" (p. xv). The author is sensitive to the complexity of southern history, and he emphasizes conflict within the South as well as the recurring antagonism between the section and the larger nation. He is particularly effective in dealing with race relations, the politics of race, and the place of blacks in southern life. Another notable feature of the work is Newby's cogent discussion of the literature of southern history and

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his successful integration of this commentary into his unfolding narrative. It is clear from his appraisal of this extensive scholarship that "the South is an active and exciting subject of historical study" (p. xiv).

Some readers may share the reviewer's opinion that, even in terms of Newby's sectional definition of southern history, some important themes are inadequately developed. These include the region's urbanization and economic transformation during the last half-century, the character of Appalachia and certain other subcultural communities, and the role of the South in the larger context of modern American politics. Even so, *The South: A History* is a fresh and authoritative synthesis, characterized by reliable handling of historical evidence, thoughtful analysis, and many illuminating insights. It provides, as the author intended, an up-to-date introduction to the history of the American South and a well-organized and clearly written account of the experience of the nation's most conspicuous section. It should prove especially useful to college students and other readers interested in a relatively brief survey of southern history.

DEWEY W. GRANTHAM
Vanderbilt University

RICHARD BEALE DAVIS. *Intellectual Life in the Colonial South, 1585–1763.* In three volumes. Knoxville: University of Tennessee Press. 1978. Pp. xxxi, 488; 491–1112; 1117–1810. $60.00.

A native Virginian who this year celebrates his seventy-third birthday, Richard Beale Davis has taught American literature in the South since the early 1930s. This beautifully crafted three-volume set, twenty-five years in the making, has won him the 1979 National Book Award in history and places the capstone on a fruitful scholarly career.

Davis surely deserves every honor for industry. In nearly 1,800 pages of text and notes he treats seventeenth-century promotional tracts and early southern chronicles, white references to the Indian, efforts at primary and higher education in the colonial southeast, the book collections of prominent southerners, printing establishments and their products, surveys in natural history, southern medicine, technology, cartography, art, architecture, music, theatre, and agricultural practice. Nor does Davis neglect topics like southern prose and poetry, letter writing, polemical pamphleteering, satirizing, "the economic factor," legal texts, study abroad, political theory, and miscellaneous oratory.

Panoramic, Davis's view is also sympathetic. He aims more than one shaft at Henry Adams, who looked down his nose at the southern mind, and takes exception to the Harvard dons of history and literature—Wendell, Murdock, Miller, Morison—who with others "have examined and explicated the New England mind with the assumption that it is the root, the fertile seed, of the living plant which is American cerebral activity" (p. xxiii). Davis intends to redress this imbalance. The colonial southerner (when southern became Southern he does not ponder) "had a mind contemplative and introspective and articulate and creative and hedonistic and observant . . ." (p. xxvi). Before 1763, concludes Davis, southern Americans, especially the leaders of that region, were "much more broadly based intellectually than has generally been recognized . . ." (p. 1,635); noting the debt that American culture owes to the South, Davis at the same time points out that many elements of Southern life and literature—pride in place, sense of family and history, love of oratory and play, gnawing moralism—have antecedents in the pre-Revolutionary experience.

Davis hopes to offset sectional prejudice in intellectual history. Ironically his volumes may reinforce stereotypes of "Southern thought": nay-sayers will note that, in many ways, his book is unrigorous, filiopietistic, and longwinded. It is generally convincing without being intellectually exciting. Few readers who wade through all these pages will doubt that there were colonials along the Southeast coast who knew their Shakespeare, kept up with the *Tatler* and *Spectator*, and were capable of pithy personal letters. Despite many architectural losses over the years there remain, Davis argues successfully, "numerous evidences in buildings of all sizes, sacred and secular, that the southern colonial enjoyed form and color and image" (p. 1,121). It may well be that one sees in the Pocohantas legend early signs of the Southern fondness for mythical womanhood or in Captain John Smith's attention to southern climate the germs of U. B. Phillips's brand of history. Because seventeenth-century southern commentary on the land portrayed it more as a paradise than as the wilderness that New Englanders saw, Davis may be correct that therein lay the roots of the "hedonism" later expressed in the writing of William Faulkner and Robert Penn Warren. Perhaps there were five styles of southern preaching.

The leisurely pace and interim objectives of Davis's work ("This is an assemblage," he confesses at the outset, "a reportage, when possible an interpretation of documentary facts" [p. xxvi]) do little to force conceptual issues. Who and what was truly "Southern?" one wonders. What exactly does he mean by "intellectual"; what does "literature" encompass? It may have helped Davis's cause to have distinguished between intellectual life and cultural style—as for example when he mentions the planters' fondness for their own portraits—and to have avoided terms like "cerebral activity" and "mental character," which hardly help us to understand dif-

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ferences among ideas, beliefs, modes of thought, practical horse-sense, esthetic preferences, and otherwise the nonmanual in life. How much "rich erudition" (p. 328) does it take to give one an intellectual bent? What of anti-intellectualism in southern life? Given criticism of Perry Miller's approach and recent essays in the theory of intellectual history, how much sense does it make to speak of "the colonial southern mind" in the first place? In Davis's encyclopedic and bibliographic manner such questions never surface and everything from British land policy to newspaper advertisements to "King" Carter's Scottish cooper somehow reflected or contributed to colonial southern intellectual life, leaving the book flat where it should be most prominent. Davis's discussion of southern religion will not displace Donald Mathews's recent work; his treatment of political theory is broken up and sketchy.

Reading colonial impressions of the Indian, Davis is sensitive and insightful; his brief account of the late colonial debate over classical versus "practical" schooling is highly interesting; his suggestion that historians compare northern and southern literary genres like elegies well taken. One quite properly salutes those few scholars who have the energy and patience to tackle so many topics, and we can hope that this example of painstaking research will inspire later laborers in what should be known as Davis's vineyard. Younger colleagues, grown soft writing intensive studies of rather manageable subjects, nonetheless will find these volumes more remarkable for their breadth than depth.

ROBERT J. BRUGGER
Harvard University

JOHN A. MUNROE. *Colonial Delaware: A History.* (A History of the American Colonies.) Millwood, N.Y.: KTO Press. 1978. Pp. xvii, 292. $17.00.

There never really has been any logical reason for Delaware's separate existence. Geographically it belongs, as the Calvert proprietors of Maryland claimed, to the eastern shore of Maryland. An odd, small piece of land, it has had a history of ambiguity and obscurity among its larger and more powerful neighbors. Yet, as John A. Munroe shows in this volume, one of a series of histories of the thirteen colonies, Delaware's beginnings make an interesting story. The number of its seventeenth-century claimants might suggest, indeed, that it was one of the world's most sought-after properties. Contested for by Swedes, Dutch, and English and, after the Dutch lost to England, fought over by New York, Maryland, and Pennsylvania, the area was finally acquired by William Penn from the Duke of York's New York grant. Under Penn, Delaware became a pendant attached to his grander holdings to the north, although his title to its government was disputed.

As Munroe reveals, Delaware's uncertain position and its separation from the bulk of Penn's territory in Pennsylvania worked to its advantage. Obscurity and confusion about the title and even its boundaries allowed the Delaware counties to escape from either close royal or proprietary administrative attention. Emerging as a semi-autonomous colony under its own legislature in the eighteenth century, Delaware was not afflicted by the battles between Quakers and proprietors that disturbed Pennsylvania, and Delawareans were not unhappy with their light proprietary rule. They also escaped having to submit their laws to England for review by the Privy Council. Left much to themselves, the citizens of Delaware increased and prospered, exploiting good soil and easy access to the Delaware River trade highway. The territory attracted large numbers of immigrants in the eighteenth century, especially Scotch-Irish and black and white Marylanders who moved across the line to Delaware's more attractive circumstances. It was a varied population, containing remnants of the original Swedish, Finnish, and Dutch settlers together with the numerous English and Scotch-Irish additions. Mixed in religion as well as economy, Delaware became both a seat of Methodist influence and a center of a varied agriculture. Tobacco early gave its economy a boost and then was supplanted by grains and milling facilities on its rivers in the eighteenth century. Although functioning as a separate political unit under the Penns, Delaware remained an economic and cultural statellite of Philadelphia. Yet this close connection did not prevent the emergence of a society with its own character—more English and Anglican and less Quaker and German than Pennsylvania.

Much of the book is devoted to the settlings, politics, and battles between claimants to the territory in the seventeenth and early eighteenth centuries. There is also a useful section describing eighteenth-century economic and social development that demonstrates how a remarkably prosperous, harmonious, and complex society grew, helped in part because it was left alone in relative obscurity. Munroe tells the story well. However, unless one is well acquainted with the particulars of Delaware valley and Chesapeake Bay geography, following all the settlings and conflicts over possession and boundaries will be difficult. Full maps with both present and colonial names for the creeks, inlets, villages, and all the other places so frequently cited are very much needed.

WILLIAM S. HANNA
University of Oregon

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JOHN F. BERENS. *Providence and Patriotism in Early America, 1640–1815.* Charlottesville: University Press of Virginia. 1978. Pp. xi, 188. $9.75.

In the last fifteen years, scholars have written various studies of American religious and intellectual history that argue the importance of millennialism in early American experience. They have not always agreed on the character of millennialism or on the ways in which it entered Americans' conceptions of themselves. Alan Heimert, for example, has emphasized the importance of the Great Awakening, while Nathan Hatch has pointed to the Anglo-French wars of the mid-eighteenth century as the decisive events through which transformations of millennial thought occurred. John F. Berens has not attempted to reconcile these or other historiographical differences; rather, he has chosen to use the insights of several scholars, including Heimert and Hatch, and to define his subject for himself. His subject is providential thought, that is, "the welter of religious-nationalist ideas articulated in post–1740 America." Millennialism appears explicitly in his study only when the writers he analyzes explicitly connect providential rhetoric and the millennium.

Providential thought is different from millennialism, "civil millennialism," the idea of American mission, and nationalism. Providential thought draws upon conceptions of America as the New Israel, the preaching of jeremiads, millennial and national expectations, and providential history. In the eighteenth century it assumed its decisive form, becoming something akin to an ideology widely shared in the young republic. By the time of the War of 1812, it was at the heart of an American consensus. That war gave it such solidity that for most of the period before 1850 it provided the key symbols of American nationalism.

Berens explains that the sources of the providential consensus lay in seventeenth-century Puritanism in New England. The Puritans thought of their settlements as the New Israel, and as they took hold of the new land they came to understand their mission as the working out of providential history, which would end gloriously with the millennium. Their children exalted the founders and made increasing use of the jeremiad, a sermonic device that allowed them to hope for the future and to continue what they were doing even as it castigated them for their sins. This body of thought, Berens argues, became an American possession, mainly through the common experience of the Great Awakening, an event that "transferred" providential conceptions to colonies further south. The actual means of the transfer were transplanted New Englanders and itinerant ministers.

What was transferred departed from the original vision of the Puritan fathers, and much of Berens' book reviews the eighteenth- and early nineteenth-century changes. During this period, providential thought absorbed political meanings. The first dramatic changes occurred during the Anglo-French wars of the mid-eighteenth century, when the identification of civil with religious liberty appeared with great clarity. From the crisis of the Revolution to the War of 1812, events were fitted into a providential framework. Berens shows in his discussion of the Revolution that providential thought played an important part in providing the myths and symbols of American nationalism. He may overstate his case, but what he notes about the persistence—and the adaptability—of providential thought is generally convincing. His discussion of revolutionary versions of American mission—of the new republic as a model, a rising empire, and an asylum for the oppressed—is skillfully presented.

Berens says sensible things about the partisan uses of providential rhetoric. Both patriots and loyalists employed providential ideas in making their cases, as Republicans and Federalists did in the 1790s. Although Berens does not suggest that the adoption of providential language by Federalists and Republicans involved a cynical play on traditional American ideas, one cannot read this book without at least wondering about the depth of party convictions.

ROBERT MIDDLEKAUFF
University of California, Berkeley

ROBERT ARCHIBALD. *The Economic Aspects of the California Missions.* Washington, D.C.: Academy of American Franciscan History. 1978. Pp. xiii, 196. $17.50.

This study fills an important void in the history of California during the Spanish era, 1769–1821. It is the first attempt to provide a detailed examination of the role the missions played in Hispanic California's economic development. It shows that the Franciscan missions were indispensable in the survival and maintenance of the civil and military populace; indeed, that thanks to the missions' prosperity the colony was almost self-sufficient by 1810, when the Mexican revolt disrupted Spanish control.

The contours of Spanish economic policy applicable to the missions were articulated in the *reglamentos* of Echeveste (1773) and Neve (1781), with later minor modifications. In the early decades, the missions were plagued by price regulations fixed by governmental fiat and the tenuous, poorly managed, but indispensable supply service operating from the Mexican port of San Blas. Weathering the resultant hardships created friction between the missionaries and local authorities. That conflict is treated judiciously.

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Nurtured by support from the Pious Fund, the missions moved from foundlings to mature institutions; at the same time, the extent of their enterprises also expanded. The very nature of their task, the Christianization and Hispanicization of the Indian, demanded a viable and flourishing economic base. In achieving that end, the missions had to diversify, experiment, and steadily improve all aspects of their activities and production capacities, particularly in crafted goods, increased crop yields, and livestock. The missions' contributions to agriculture were harbingers of California's agribusiness. Although the colony was handicapped by a lack of specie, the creation of a barter economy did not deter development. Within a scant few years, "California passed from a struggling frontier bastion to virtual economic independence . . ." (p. 183).

There are, however, indications that the missions had probably reached their economic apogee by 1821 and were doomed to decline. The diminution of the Indian labor force, so essential to mission productivity, was already a problem. Increased foreign contact, bringing with it a new element of competition, and an unenterprising populace, as well as the absence of technological innovation, also suggest that future prospects were bleak. Still, there is no discounting the missions' economic success up to 1821.

Robert Archibald tells that dramatic achievement in eight tightly written chapters that always shun provincialism. Heavily documented, with extensive reliance on manuscript sources, replete with twenty-two tables, this book is a solid contribution to the literature.

DOYCE B. NUNIS, JR.
University of Southern California

ANNE Y. ZIMMER. *Jonathan Boucher: Loyalist in Exile.* Detroit: Wayne State University Press. 1978. Pp. 395. $17.95.

Anne Y. Zimmer's *Jonathan Boucher* is a straightforward, old-fashioned biography that does not pretend to be anything else. In clear prose it recounts the three separate careers of the man Zimmer calls "the leading Tory figure," at least in terms of his "intellectual capacity and ability to philosophize" (p. 9). Boucher was first an aspiring English cleric and schoolmaster, then a colonial parson, and finally, again in Britain, an exile who developed a penchant for lexicography and local history. Zimmer has performed a particular service through her attention to Boucher's little-known years after his return to his native land, showing how he slowly adjusted to his loss of American status and how he successfully reintegrated himself into the society he thought he had left for good almost two decades previously.

But most readers will be interested primarily in Zimmer's discussion of Boucher's years in America, for his chief claim to prominence derives from them. Boucher arrived in the colonies in 1759 at the age of twenty-one, having accepted the post of tutor to the children of a wealthy Virginian. Through ambition, determination, careful politicking, and no small measure of luck, Boucher over the course of the next fifteen years rose to become a successful clergyman and planter in the neighboring province of Maryland. Zimmer tells the story of that ascent well: its twists and turns, its feuds, failures, and triumphs are chronicled in detail and can serve historians as an epitome of the inner workings of colonial clerical place hunting.

Unfortunately, Zimmer's treatment of Boucher's political thought during this crucial period is less satisfactory. She does usefully identify some of his heretofore anonymous essays, and she further amplifies the position she and Alfred Kelly advanced some years ago, that Boucher from the mid-1760s through 1773 was in sympathy with the American resistance movement despite his subsequent conservative image. But for the most part her consideration of Boucher's political views consists of lengthy quotes and paraphrases from his writings. It seems almost as though, confronted by a plethora of material, she had difficulty knowing what to make of it all. One common mistake she carefully avoids: she does not read Boucher's *A View of the Causes and Consequences of the American Revolution*, published in 1797 and purporting to be a collection of his prerevolutionary sermons, as indicative of his thought in the decade 1763–73. Instead, she correctly places it in the context of Boucher's late-developing high Tory philosophy. One would hope that scholars will now recognize that Boucher was being disingenuous, if not patently deceptive, when he claimed that he had reproduced the sermons "very nearly as they were delivered from the pulpit" (*A View*, p. lxxxv).

All in all, Zimmer's biography is solid and workmanlike. She does not provide readers with psychological insights into Boucher's mind or behavior, but she aptly and accurately summarizes his career. For this accomplishment all historians of the loyalists owe her a vote of thanks.

MARY BETH NORTON
Cornell University

MALCOLM LESTER. *Anthony Merry* Redivivus: *A Reappraisal of the British Minister to the United States, 1803–06.* Charlottesville: University Press of Virginia. 1978. Pp. x, 161. $10.00.

Shortly after he arrived in Washington in November 1803, Anthony Merry, Great Britain's new en-

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voy to the United States, was disturbed when President Thomas Jefferson was absent when he was supposed to receive the Englishman. Merry was more annoyed when he met Jefferson, for the Virginian was dressed informally. Attending a function at James Madison's residence the same day, Merry and his wife were scandalized when the secretary of state offered his arm to Mrs. Albert Gallatin instead of Elizabeth Merry. Merry's ministership, which began inauspiciously, never improved; he (and his wife) had many complaints: servants were hard to obtain and hold; American cuisine left much to be desired; Washington was a small community without the cultural amenities of European cities; Merry's health was poor (he suffered acutely from hemorrhoids).

Historians have not neglected Merry or his troubles. Every account of Jefferson in power devotes space to "toujours gai," as Merry was dubbed by Napoleon Bonaparte, and the Briton has been described by Claude Bowers as a "pompous mediocrity" (p. 1), by Richard Beale Davis as a "thin-skinned almost stupid man" (p. 1), by Reginald Horsman as "mediocre" (*The Causes of the War of 1812* [p. 47]), and by Marshall Smelser as "a bit of a dunce" (p. 1).

To Malcolm Lester, whose work is the outgrowth of a seminar taken three decades ago under Bernard Mayo, these verdicts are unfair. In this brief study, which is almost exclusively concerned with Merry's American mission and which uses standard sources in lieu of a Merry collection, Lester has reconsidered Anthony Merry and his career in America. According to Lester, "toujours gai" was a victim of his heritage and circumstances: the forty-seven-year-old Merry came to the United States following undistinguished diplomatic service in Europe, especially in tradition-bound Spain where his zest for detail and protocol was an asset. Having been exposed to the world of commerce from his childhood, Merry defended Britain's commercial practices and upheld the Royal Navy's actions on the high seas. Unhappily for the English diplomat, his arrival in the United States nearly coincided with two events with negative implications for Anglo-American relations: the Louisiana Purchase, which lessened American fears of France, and the resumption of hostilities between Great Britain and France. Finally, conditions in Washington were primitive and the Merrys (the obnoxious Elizabeth, especially) were unprepared for the hardships they encountered.

Historians will have to take Lester's workmanlike defense of Merry into account. Still it seems unlikely that future appraisals of the English representative will differ radically from past assessments, for even Lester admits that the cautious Merry had major deficiencies—ranging from an inability to comprehend the United States to relying excessively on Jefferson's Federalist foes for information. If Anthony Merry was not a "dunce" or a "pompous mediocrity," he was the wrong man in the wrong post at the wrong time.

CLIFFORD L. EGAN
University of Houston

DUANE H. KING, editor. *The Cherokee Indian Nation: A Troubled History.* Knoxville: University of Tennessee Press. 1979. Pp. xix, 256. $12.50.

In this volume Duane H. King, director of the Museum of the Cherokee Indian, attempts to give an overview of Cherokee history through the essays of twelve experts on various subjects related to tribal history. The editor, in his introduction, briefly recounts the major events of Cherokee history and places each selection within the larger context. The essays that follow are arranged in rough chronological order and discuss such topics as tribal prehistory, the evolution of law and government in the period prior to 1828, slavery, the political factionalism encountered after the removal, and certain recent events such as the militant Five County Cherokee Organization in Oklahoma. Although the dust cover proclaims that this collection of essays "offers new insights into the troubled history of these Native Americans," such is not the case.

Taken as a whole, *The Cherokee Indian Nation* offers the reader neither an overview of tribal history nor any significant amount of new information. Even the subtitle, "A Troubled History," applies to only a fraction of the book. The main difficulty stems from the varying quality of the contributions and the question of their relevancy to the overall theme of the book. Some of the essays are good, solid pieces of scholarship that broaden our knowledge of Cherokee history. Others, however, are of lesser quality, and some have no observable place in such a limited work. Two examples will suffice. One article is an analysis of some eighteenth-century maps that mark the location of various Cherokee towns. The charts and tables are uninspiring, and the subject, which is of little interest to anyone except the most devoted student of tribal trivia, does not stir the reader. Another lengthy article is on the career of William A. Bowles, a white man who posed as a Cherokee in Europe during the time of the French Revolution. Although this essay is well researched and written, it has little place in this work, despite the editor's claim that it presents an "episode of Cherokee involvement in international diplomacy" (p. xv).

The most valuable aspect of this book thus comes from several of the individual essays that clarify and enhance certain features of tribal history. Among

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these are the two articles that discuss the development of law and government and the one on factionalism between the Ross and Treaty parties in the post-removal period. These are well-researched, original pieces of writing. Unfortunately, from the viewpoint of a historian, even the good material fails to salvage this book. The field of Cherokee history is already oversaturated, and *The Cherokee Indian Nation* only adds to that conclusion.

ROBERT A. TRENNERT, JR.
Arizona State University

JOHN D. UNRUH, JR. *The Plains Across: The Overland Emigrants and the Trans-Mississippi West, 1840–60.* Urbana: University of Illinois Press. 1979. Pp. xviii, 565. $22.50.

Of the many books that pass across a reviewer's desk, two types are the most dreaded: those for which little of a positive nature can be said and those about which only superlatives apply. Reviewing the former is unpleasant for reviewer and author alike; reviewing the latter is an arduous task if the review is not to read like a publisher's dust jacket blurb. *The Plains Across* falls into the latter category, and, in spite of the pitfalls of the highly laudatory review, it must be said at the outset that this is a superb book. Many clichés are relevant—seminal work, definitive study, formidable scholarship. To have them apply to a volume that is, at the same time, skillfully and sensitively written, lavishly illustrated, and admirably documented is truly remarkable.

The Plains Across is, by the author's intent and admission, a revisionist study of the great American folk migration to the Pacific. Virtually all of the stereotypes of that migration that have been common fare for the media and conventional wisdom for some historians have been meticulously and convincingly dissected by John D. Unruh, Jr., and replaced by more valid interpretations. Debunking of myth is nothing new to historical scholarship; many contemporary Western historians have, for some years, been discussing such facets of the antebellum migrations as Indian-white relationships in terms that are more rational than the traditional TV-movie-novel image of the savage redman pillaging a defenseless wagon train. What makes Unruh's work so important is that he erases most of the other myths of the migration as well as this particular one, that he does so on the basis of sound scholarship and accurate data, and, most important of all, that he leaves us with something to take the place of the erased sterotypes. The replacement derives from the organizing principles of the author's research and of the book itself: first, that the migrations cannot be discussed in terms of "typical" years such as 1843, 1846, or 1849, but must be treated as a phenomenon that changed with time; and second, that the success of the migrants was based largely on cooperation and interaction between the migrants and many temporary and permanent inhabitants of the West. Thus the myth of the marauding Indian is supplanted by the details of Indian assistance as well as hindrance. And thus is the myth of the solitary wagon train, isolated and alone on the vast inland plains, replaced by the picture of migrants cooperating with one another, with private entrepreneurs, federal agencies, the military, and those already settled in the West, in order to secure the success of their migrations.

The book begins with a detailed examination of the historiography of the migrations, followed by an excellent analysis of public attitudes toward the West itself and the ease—or the difficulty—of going there and by an intriguing look at the role of promotion in motivating the migrants and at the developing logistics of transcontinental travel. The heart of the book, however, is the study of the interactions between the migrants themselves and with the Indians, the federal government, and private enterprise. An excellent summary places the migrations in historical perspective, reiterates the organizing principles, and details the sources upon which the work was erected. It is at this point that one would see the author as developing additional works out of this beginning study. Such, alas, is not to be the case. Unruh died prior to the book's publication and Western history has lost his potential contributions. But the present contribution—*The Plains Across*—is a magnificent one and will be a directing force for years to come. Let the words of an Oregon pioneer, speaking of the migration, stand as epitaph for both the migrations and the author: "The bravery and hardihood of the effort was only equalled by the substantial fruits of the achievement."

JOHN LOGAN ALLEN
University of Connecticut

PETER F. WALKER *Moral Choices: Memory, Desire, and Imagination in Nineteenth-Century American Abolition.* Baton Rouge: Louisiana State University Press. 1978. Pp. xxii, 387. $24.95.

Rich in original analysis, *Moral Choices* is also a work of art. A short review cannot possibly do justice to Peter F. Walker's sparkling intelligence and sensitivity to language. Nor can a short review do more than mention certain disturbing flaws. Walker begins with what seems to be a simple question: Why did certain people become abolitionists? But he approaches the question with a finely tuned understanding of psychological process and with a skilled

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eye for textual analysis. The book's subtitle, stressing "memory," "desire," and "imagination," refers to autobiography, "an imaginative response to internal necessities, whereby a self-writer has imposed his creative powers on the data of his life" (p. xviii). For Walker, an individual's commitment to a moral cause reveals extraordinary complexities—especially those involving relations with a father or cultural embodiments of fatherhood (Walker by no means ignores motherhood, but relegates it to a different level). For interesting and justifiable reasons, Walker focuses most of his attention on three figures who were somewhat "marginal" to the mainline abolition movement: Moncure Conway, who finally made an irreparable break with his slaveholding but liberal-minded father; Jane Swisshelm, who as the daughter of a sectarian Scottish Covenanter "was a believer in a Garrisonian doctrine before there was such a thing; by the time it began to develop she was moving in the opposite direction" (p. 104); and Frederick Douglass, who when nearly eighty sought out a descendant of his old master in order to record some dates and facts that might illuminate his own identity and white paternity.

The second part of *Moral Choices* also consists of biographical studies (Henry Wright, Salmon Chase, and Thomas Cooley). But Walker's purpose, now "frankly elliptical and provisional" (p. xxi), shifts to a larger screen. Earlier chapters point to many bridges between personal needs and the wider social meanings of abolitionism. For example, in the most brilliant stroke in the book, Walker shows how the abolitionists' supposed obsessions with guilt and anti-institutionalism are thrown into question by their furious repudiation of Conway, who in 1863 "asked them to be true to their own words" (p. 18), that is, to their earlier rhetoric of disunion and nonviolence. Conway's traitorous proposal, made in England, was to link Southern independence with a guaranteed and internationally supervised emancipation of the slaves. The resulting uproar revealed an underlying commitment to nationalism, "a belief in the curative, restorative, and redemptive quality of America" (p. 19)—a theme Walker tries to develop further in later chapters. Yet he is far more successful with individual biography than with "dove-tailing" individual lives with such broad themes as unionism, accommodation, and laissez-faire ideology.

Walker's judgments will provoke controversy. He displays a clear animus toward "the common anti-slavery line" and toward the mentality of reformers like Jane Swisshelm. He overemphasizes the connections between abolitionism and Manchesterian economics. Yet the prudent reader should not miss Walker's qualifications, often consigned to footnotes, or be put off by fanciful speculation or by occasional slips, such as the misspelling of "millennial" and repeated reference to the last book of the Bible as *Revelations* (p. 140). For no other book has probed so deeply into the "self-creating impulse" of individual abolitionists or has done more to enrich our understanding of the limits of their "imaginative social desires."

DAVID BRION DAVIS
Yale University

BLANCHE GLASSMAN HERSH. *The Slavery of Sex: Feminist-Abolitionists in America.* Urbana: University of Illinois Press, 1978. Pp. xi, 280. $14.50.

Originally a dissertation, this topical overview linking nineteenth-century feminism and antislavery reworks familiar ground. In its disparate chapters, Blanche Glassman Hersh ranges from reform activism to feminist ideology to collective biography. It is her chapter on "Husbands in the Women's Movement," however, that contributes most either to women's or reform history.

The first of the two central biographical chapters attempts, by concentrating on Lucy Stone, Elizabeth Cady Stanton, and Susan B. Anthony, to define "all the influences that converged in the antebellum period to provide the impetus for a women's movement" (p. 114) and to establish that movement's diversity. Because the three were chosen for prominence rather than variety and analyzed with little attention to personality, this brief treatment of their careers simply does not establish the relative importance of liberal theology, heightened educational and occupational expectations, radical abolitionism, and "feminist consciousness-raising" (p. 114) in shaping nineteenth-century women's activism.

More disappointing because potentially more revealing is Hersh's profile of fifty-one feminist abolitionists. Again, her principles of selection, never explicitly addressed, create problems. Why, for instance, are two women of central European birth included, because of their striking differences from native and English-born feminists, while American-born blacks are consciously excluded? (Indeed, Hersh lauds Abby Hopper Gibbons for her membership in an otherwise all black, female antislavery society without considering what such a society's very existence implied about her earlier assertion that women abolitionists identified with female slaves as sisters.)

The subsequent analysis raises even more substantive questions. Hersh uses the New England birth of twenty-four and the ancestry of thirty-three women to claim that their New England heritage, embracing a "nationalistic pride in their Yankee roots and a Puritan sense of duty" (p. 121) as well as a "brand of class consciousness" (p. 94) deriving

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from "aristocratic soil" (p. 93), determined women's reform activity. Even if one does not argue with the terms ascribed, one must note that Hersch's case rests on the implicit assumption that heredity dominates environment. For, of the first twenty-five women in her sample, ten were indeed born and reared in three New England states. But an equal number grew up in western New York and Ohio, scenes of the religious revivalism that Hersh argues was, along with education, the major force singling out activist women from their neighbors.

Finally, Hersh's analysis of feminist ideology lacks a broader historical context. Denying, for instance, that William O'Neill's division of later feminists into "hard core" suffragist-oriented and "social" moral-reform-oriented categories can be applied to the first generation, she makes her point by ignoring educational reformers like Emma Willard, Mary Lyon, Zilpah Grant, and Catharine Beecher. Likewise, in evaluating feminist theories of health, she refers not at all to Thomsonianism and homeopathy, which similarly challenged heroic medicine.

Had she written a probing comparison of a few feminists, undertaken a systematic prosopographical study, or analyzed the interrelationship between antislavery and feminist ideas as Anne Douglass and Carroll Smith-Rosenberg have synthesized other aspects of nineteenth-century middle culture, Hersh could have made a major contribution. But in attempting a smattering of each, she has disserved herself and her readers.

JANE H. PEASE
University of Maine, Orono

AL-TONY GILMORE, editor. *Revisiting Blassingame's* The Slave Community: *The Scholars Respond.* (Contributions in Afro-American and African Studies, number 37.) Westport, Conn.: Greenwood Press. 1978. Pp. xvi, 206. $15.95.

In 1972 John Blassingame published *The Slave Community: Plantation Life in the Antebellum South*, a provocative book that elicited a range of critical responses, from denunciations that the book was a mundane assortment of the familiar to laudatory proclamations about its originality and scope. Six years later, following in the tradition of Stanley Elkins's *Slavery* and William Styron's *Confessions of Nat Turner*, appears *Revisiting Blassingame's* The Slave Community: *The Scholars Respond.* Edited by Al-Tony Gilmore, the volume evolved from a session on *The Slave Community* held in 1976 at a meeting of the Association for the Study of Afro-American Life and History; it incorporates observations from nine scholars, many of whom participated in the conference, and concludes with John Blassingame's comments upon the commentators.

It is a curious and interesting book, as much for its tone and omissions as for what it says directly. All the contributors agree that, although the book has many flaws, its framework and approach remain of central concern to scholars, but no two authors agree as to what the flaws are or even on the contributions for which Blassingame should be credited. It is clear that the issues surrounding American slavery are as elusive as ever, perhaps more so. The contributors recognize that the overriding value of *The Slave Community* is its examination of slavery from the perspective of the slave, thus placing it in a significant and growing body of work that assumes, as one author put it, that "the source of black culture and black struggle is the black community itself" (p. 17).

The collection opens with Mary Frances Berry's review of the reviews, a judicious and useful summary of the original responses to *The Slave Community*. George Rawick, beginning with the statement that, "like many good books, it should have been better" (p. 26), criticizes the neglect of black women in Blassingame's treatment and the "undialectical" assessment of the slave personality. Berry claims that *The Slave Community* "helped to lay the Elkins thesis to rest" (p. 6), and Ralph D. Carter asserts that the book did "an admirable job of demolishing the thesis" of the Elkins book (p. 74), but James D. Anderson argues that Blassingame accepted too much of the "Elkins-Moynihan conception" of slave pathology.

Eugene D. Genovese, while pleased that Blassingame used a psychological model in his study of slave personality, argues that the weaknesses of the book are a product of Blassingame's use of Sullivanian, rather than Freudian, psychology. Leslie Howard Owens finds major fault with the use of psychological models at all, insisting that Blassingame should have built exclusively on slave narratives.

Much is made of the use, or lack of it, of slave sources. Rawick and Earl E. Thorpe, to whom the book is dedicated, criticize the neglect of 2,000 WPA interviews of the former slaves, edited in nineteen volumes by Rawick, and Stanley Engerman addresses the problems inherent in the reliance on slave narratives as well as Blassingame's handling of them. But Ralph Carter thinks *The Slave Community* is strong because of its extensive use of slave narratives. Engerman, in one of the most thoughtful essays in the book, views *The Slave Community* in the context in which it appeared and with the recognition of the enormous impact that Elkins's work had on the entire subject, and he concludes that Blassingame presented a "basically plausible and consis-

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tent interpretation of [the] interrelationship of slave and master" (p. 101).

The contributors have a commitment to honest criticism, but they seem to be uncomfortable with the seriousness of that criticism. Rawick deals with his unease by saying that "our intention is not to . . . fulsomely praise a particular book but to further a common task and a common body of work" (p. 14). Genovese says that Blassingame "has given us a book that transcends its own deficiencies. It is a book we can build on" (p. 40). Owens uses Blassingame's book to "reflect together upon what we know, or perhaps more importantly, don't know, about the contours of our forefathers' lives" (p. 61). Engerman discusses Blassingame's book as transitional.

The only person in this volume who seems comfortable with the limitations and strengths of *The Slave Community* is John Blassingame, whose response to his critics is amiable. He denies the validity of many charges, but engagingly admits to many others. He neglected the WPA interviews, he says, out of "intellectual timidity and methodological conservatism," feeling unsure of how to develop a "reasonably critical apparatus" for their use (p. 147). (The conciliatory tone of his essay contrasts with an article he originally published in 1975, reprinted in this volume, where he argued that the interviews were not usable. His willingness to have both pieces side-by-side is admirable.) He recognizes the neglect of black women, saying that the book was "written by a man using language replete with male images and was based on sources written largely by males" (p. 140). If he created confusion where there had been clarity, he concludes, he hopes that it was "creative confusion."

An observation by Genovese characterizes the spirit of this volume: Blassingame "deserves the engaged criticism that provides the only genuine tribute one scholar can offer another" (p. 27).

ANN J. LANE
The Bunting Institute, Radcliffe College

WILLIAM J. COOPER, JR. *The South and the Politics of Slavery, 1828–1856.* Baton Rouge: Louisiana State University Press. 1978. Pp. xv, 401. $22.50.

William J. Cooper, Jr., has furnished a very detailed treatment of the South's involvement in national politics from 1828 through 1856 with steady focus on one theme, "slavery as the fulcrum of southern politics" (p. xi). The South is defined as the eleven states effectively in the Confederacy, and the period is that of the second American party system. The thesis is that a politics of slavery prevailed in the South that rendered contests between southern Democrats and southern Whigs merely a function of the "relations between those parties and their northern counterparts" (p. xiii). The author argues, and illustrates for each presidential contest, that the viability of each southern party depended upon its apparent capacity to win from its northern wing an adequate recognition of southern rights with regard to slavery. Slavery is offered as a surrogate for southern honor and self-respect, and the politics of slavery as a result of interaction of slavery, the political system, and the values of southern white society. To southerners, parties were not instruments for dealing with economic, ethnic, or religious concerns but were principally guardians of their rights in the nation.

The original emergence of the Whig party as a serious contender in the South in 1836 is presented as a process of establishing an image as a better defender of slavery than the Democrats, who nominated a northern man for the presidency that year. Each of the five subsequent presidential contests is analyzed in terms of changing circumstances but constant concern with the defense of slavery. The brief interval when southern Whigs supported Henry Clay against President John Tyler on economic issues is dismissed as "The Great Aberration" in a chapter by that title. The narrative concerns familiar political developments but rests on very extensive use of newspapers, manuscripts, and published correspondence. The episodes are recounted with skill and enriched with pithy contemporary comment, yielding a plausible, massively documented case. Because the author is offering his own assessment of voluminous sources previously studied by many other scholars whose conclusions are incompatible with his view, the work is principally a cogent interpretive essay, lavishly illustrated. It probably will fail to convince those who doubt that southern white people were so united or absorbed in slavery concerns or who do not agree that other political considerations were as insignificant as they are made to appear in this book. Selection of evidence in support of any theme from so vast a body of primary materials as exists for this topic would necessarily encounter the same hazard. Nevertheless, anyone seriously studying southern politics must deal with this lucid and painstaking presentation.

THOMAS B. ALEXANDER
University of Missouri, Columbia

JOHN F. STOVER. *Iron Road to the West: American Railroads in the 1850s.* New York: Columbia University Press. 1978. Pp. xii, 266. $14.95.

In the decade of the 1850s, the United States railroad sector was a major factor in the pattern of eco-

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nomic growth. Expansion of the railroad net went forward rapidly, especially in the Old Northwest, and many of the institutional forms and patterns that would dominate American railroad enterprise in future years emerged in this period.

Given the importance of the fifties in American railroad history (and economic history), there is ample room for a narrative and compilation of the basic data on this decade. We are given those things—narrative and a considerable amount of well-systematized data—in this work by a leading railroad historian, John F. Stover. The book is very largely written on the basis of data taken from the *American Railroad Journal*, a well-edited and remarkably thorough journal, and from data distilled from the fairly large secondary literature on individual lines and on regional transport development. Chapters on each major region summarize the railroad promotion and building of the fifties. There is one chapter devoted to retelling the well-known story of the first federal land grants and another to the subject of railroad competition with canals and steamboats. The work closes with a summary of some of the leading technological innovations (though without much of consequence on the processes of diffusion) and an overview of railroads in 1860–61.

One is surprised to find almost nothing in the way of citations to the annual reports of individual lines, and the mileage data given are taken from Poor's *Manuals*—with attention only to 1850 and 1860 as benchmark years. Consequently, Stover fails to identify patterns, either of cyclical construction or of organizational movements, that would have been partially defined by year-to-year changes in mileage built or in business strategies. No effort is made to come up with estimates of investment; indeed, railroad finance is treated only cursorily. Intriguing questions, not least the matter of whether or not railroads "led" or were "built ahead" of settlement and traffic, are simply not addressed at all, though the most important works on transport history published in the last quarter century might have made them of central importance. Thus, while Stover has provided a useful compendium of information, this is a work that in some measure will be disappointing to economic and business historians.

Columbia University Press has produced this work with the annoying uneven right margin (unjustified margin), and typographical errors are frequently found.

HARRY N. SCHEIBER
University of California, San Diego

G. S. BORITT. *Lincoln and the Economics of the American Dream.* Memphis: Memphis State University Press. 1978. Pp. xxiv, 420. $15.00.

To those scholars who ask periodically whether "the Lincoln theme has been exhausted," G. S. Boritt's book provides the response "not yet." The aspect of the Lincoln theme that Boritt has investigated is economic: his book is a study in the history of ideas, the ideas, concepts, beliefs concerning economics held by Abraham Lincoln.

Boritt sets forth two major propositions, the first of which is that ideas about economics occupied such a fundamental place in Lincoln's outlook that they provide "a major key to fathoming Lincoln" (p. viii). In support of this proposition, most of the book is a chronological description of the ideas pertaining to economics expressed by Lincoln from the time he first ran for office in 1832 until shortly before his death. Lincoln, Boritt maintains, "probably talked more about economics, to use the term in a broad sense, than any other issue, slavery included" (pp. xi, 324 n. 8). The second proposition advanced by Boritt is that biographers and historians who have written about Lincoln have, with a very few exceptions, been unaware of the important role of economics in his thought and actions and thus have ignored rather than explored the subject. This second proposition is discussed in particular in a historiographical essay printed as an appendix to the book.

The most original and substantial contribution of the book is its discussion of Lincoln during the first two decades after he entered public life, 1832 to 1854, a discussion that occupies approximately one half of the book (pp. 1–152). Space is not available to summarize that discussion in detail, but as one example it is worth noting that Boritt describes the origins of Lincoln's Whig economic ideas through analogy to present-day developing nations. Just as do people in those nations today, so did Lincoln and other early settlers in Illinois of the 1830s feel the need for government policies to assist the improvement and development of their communities through aid for transportation and banking facilities. Moreover, Boritt suggests a connection between Lincoln's Whiggish economic ideas and his fundamental social and political ideals—the "key" to Lincoln's economic ideas, Boritt writes, was "an intense and continually developing commitment to the ideal that all men should receive a full, good, and ever increasing reward for their labors so that they might have the opportunity to rise in life" (p. ix). Boritt calls these interconnected ideas "Lincoln's American Dream," and hence the phrase in the book's title, "The Economics of the American Dream."

The second half of the book, discussing the final eleven years in Lincoln's life, is less original than the first half and more vulnerable to criticism. Even in this portion of the book, however, Boritt's interpretations are fresh and interesting. Throughout the

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book, Boritt describes Lincoln and "Lincoln's American Dream" sympathetically, although not uncritically. While explicitly avowing his admiration for Lincoln, Boritt suggests that an "other-minded student" may reach from the evidence presented a less favorable or definitely hostile evaluation. The book as a whole, including its historiographical appendix, is a stimulating, provocative, and, perhaps unavoidably, controversial addition to the enormous body of writings about Abraham Lincoln.

THOMAS J. PRESSLY
University of Washington

STEPHEN B. OATES. *Our Fiery Trial: Abraham Lincoln, John Brown, and the Civil War Era.* Amherst: University of Massachusetts Press. 1979. Pp. ix, 150. $11.50.

Our Fiery Trial is a small book that packs a big wallop. It is must reading for all students of mid-nineteenth-century American society and politics. In his previous work I admired Stephen B. Oates's ability to turn a phrase and illustrate a point with a telling quote or anecdote; I viewed him as one of our better narrative historians, but one whose work was somewhat lacking in analytical rigor. This book proves that Oates has outstanding conceptual abilities.

This collection contains ten essays: one on William Styron's Nat Turner, three on John Brown (including two analyzing the work of Brown's previous interpreters); three on Lincoln (including a favorable but not uncritical appreciation of Sandburg's work); one on the "enigma" of Stephen A. Douglas; another on the "role of historical myth," that is, the abuse of history by such figures as John Wayne, Paul Harvey, and Ronald Reagan; and a final essay containing Oates's reflections on the art of history in general and biography in particular.

The essay I enjoyed most was "Lincoln's Journey to Emancipation," which I found more compelling than Oates's biography, but the main purpose of this collection is to level interpretive salvos at Styron as well as historians such as Eugene D. Genovese, Allan Nevins, C. Vann Woodward, James C. Malin, James A. Rawley, and others, including the ideologue Truman Nelson. In my opinion, Oates succeeds with telling effect. Styron, of course, viewed his *Confessions* not so much as a novel but as a "meditation on history" in which he depicted Turner as a "sexually crippled bachelor given to masturbation fantasies about white women." Oates legitimately questions Styron's utilization of "poetic licence" when he writes without fear or evidence about the life of a major historical figure. But Oates expresses astonishment, which I share, when respected and eminent members of the historical guild lavish praise and comment upon the possible accuracy of Styron's treatment. Like it or not, historians make inferences; but ordinarily, we have some—and at times compelling—evidence to buttress our conclusions. Styron's Turner clearly tells us more about Styron than Turner. We all are entitled to our fantasies, and some of them may even be interesting, but they must not be confused with truth or reality.

Oates's criticism of the views of Malin, Nevins, Nelson, and others on John Brown are equally pointed, and I agree with the emphasis he places upon perception of reality in interpreting lower South reactions to abolitionists and Republicans in general and Brown's raid in particular. Like Stephen Channing and others, he reminds us that what people *think* is true is as important as what *is* true. By lavishing praise on this book, I do not mean to suggest that all its essays are equally compelling; they are not. But, overall, the book is a remarkable achievement and deserves a wide audience.

RICHARD O. CURRY
University of Connecticut

MICHAEL C. C. ADAMS. *Our Masters the Rebels: A Speculation on Union Military Failure in the East, 1861–1865.* Cambridge: Harvard University Press. Pp. x, 256. $12.50.

This is a book of some significance. Michael C. C. Adams's thesis is that the American Civil War in the Eastern theater of operations was lengthened by an awe and fear of the aristocratic Confederates held by some Federal commanders—allegedly George B. McClellan in particular—and that this made the Southerners "masters" over the unmotivated Unionists.

The book is rather well written and contains full notes (although a number of important sources and studies were missed), but maps and a bibliography are not included. The highly selective quotations at times lead to unsubstantiated, sweeping generalities, and historians of the Civil War may be surprised at Adams's citing of the weak books on Chancellorsville by Doubleday and Stackpole while ignoring John Bigelow's masterful study. Similarly, the frequent citing (especially in chapter five) of Kenneth P. Williams's *Lincoln Finds a General* (1949–59)—the first two volumes of which were roundly criticized for poor historical methodology by such an authority as Bell Irvin Wiley—does not strengthen the author's case. Rather than suggesting *Battles and Leaders* on James Longstreet's conduct at Gettysburg, why not recommend Glenn Tucker's *Lee and Longstreet at Gettysburg* (1968)?

The author is unconvincing in his severe treatment of McClellan. For example, Adams states that

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McClellan "only fought" defensively (p. 101). This is simply not so, as McClellan initiated the assaults at South Mountain and Antietam, the latter being the war's bloodiest day. Nor is Adams on firm ground when he asserts (p. 147) that, as of mid-1863, the Army of the Potomac "had never fought offensively," when actually that army did the attacking at First Bull Run, Second Manassas, South Mountain, Antietam, and Fredericksburg. Many an eyebrow will be raised at the author's contention that U.S. Grant achieved his objective against Robert E. Lee "without excessive cost" (p. 172). And it is not true that "Grant was the first of Lincoln's generals to fully appreciate the basic need for cooperative effort on the part of the Union armies" (p. 154), for such commanders as Winfield Scott, McClellan, Henry W. Halleck, and others clearly saw this necessity. Adams contends that Grant did not have a high opinion of the Confederates, but Grant affirmed, after the battle of the Wilderness, that "Joe Johnston would have retreated after two days of such punishment."

The author is more persuasive in his treatment (chap. 8) of William T. Sherman, who fluctuated in his opinion of Confederate ability. Adams states correctly that Sherman urged slaughtering the entire Southern ruling class and actually enjoyed seeing Confederates suffer. "Cump" also inveighed against introducing—in his words—"niggers and bought recruits" into the Federal armies, as Adams notes (p. 165).

Although the verdict must be that Adams's case is unproved—especially in his claim, based on a comparatively few, highly selective quotations, that the Army of the Potomac and the Northern masses had an inferiority complex vis-à-vis the Southerners—nonetheless Civil War historians should not ignore this able, stimulating, and provocative interpretation; indeed, all doing work in the field would do well to read it.

WARREN W. HASSLER, JR.
Pennsylvania State University

PAUL D. ESCOTT. *After Secession: Jefferson Davis and the Failure of Confederate Nationalism.* Baton Rouge: Louisiana State University Press. 1978. Pp. xiv, 295. $17.50.

A few weeks after the Emancipation Proclamation was announced, Confederate Secretary of War James Seddon sent Jefferson Davis a recommendation to counteract the intense resentment among poor farmers about the "twenty nigger law." Seddon's proposal to exempt from military service poor men on whom "eight or ten helpless whites are dependent" was "one of the most important ideas advanced within the Confederate government during the war," according to Paul D. Escott, "for it had the potential of relieving much of the suffering which sapped the loyalty of the common people" (p. 152). Yet Seddon's plan to reduce tension between classes of whites received no more support from Davis than Lincoln's plan to foment unrest among slaves. Davis's insensitivity to the suffering and popular disaffection created by Confederate policies undermined southern morale and was responsible for the Confederacy "collapsing from within long before" it was defeated on the battlefield. "Thus, the greatest failure of Jefferson Davis's leadership lay in the domestic arena," Escott argues, "in his inability to create the internal unity and spirit essential for the growth of Confederate nationalism" (p. 272).

Escott's major achievement is to make clear that some of the most important battles of the war were fought well behind the lines. Making especially fruitful use of correspondence in Confederate records, Escott documents the widespread opposition among poor farmers to government policies of taxation, impressment, exemption, and substitution and to the government's indifference about many citizens' extreme poverty. In "The Quiet Rebellion of the Common People" (pp. 94–134), Escott emphasizes that rebelliousness coincided with class divisions in the south and at times rose to levels that were far from quiet, as when the First Alabama Cavalry, U.S.A.—a regiment of about two thousand whites—marched through Georgia with General Sherman. Yet Escott's insights about the class divisions within the Confederacy call into question his conclusions about Jefferson Davis.

Escott argues that Davis failed to generate an effective nationalist ideology and failed to provide relief for the suffering of poor farm families. He contrasts Davis's failure with the successes of Governor Joseph E. Brown of Georgia in sympathetically responding to the privations of the poor. But if Brown had been in Davis's shoes, would his actions have been different from Davis's? Even if Davis had acted as Escott insists he should have, was charity enough to allay the unrest among poor nonslaveholders and to create the material basis for a successful plea for national unity? The fundamental flaw in supposing that nationalism could have united the South—even if Davis had perceived that as his mission and made a concerted effort to accomplish it—is that the Confederacy was created by and for slaveholders. Given the social basis of the southern nation, Escott's argument that nationalism was the solution to social disunity is implausible. Instead, Escott's evidence of Davis's obtuseness to the social consequences of Confederate policies suggests that nationalism was the problem.

The best assessment of Davis's statesmanship remains David M. Potter's essay, "Jefferson Davis

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and the Political Factors in Confederate Defeat." Escott never confronts Potter's argument, nor does his extensive analysis of Davis's nation-building activities give any reason to dispute Potter's withering conclusion that "there is no real evidence in all the literature that Davis ever at any one time gave extended consideration to the basic question of what the South would have to do in order to win the war" (*The South and the Sectional Conflict* [1968], p. 283). Escott has made a valuable contribution to our knowledge of the nature and extent of disunity after secession. But a much clearer reading of the significance of southern nationalism is contained in the Emancipation Proclamation.

MICHAEL P. JOHNSON
University of California, Irvine

EMORY M. THOMAS. *The Confederate Nation, 1861–1865.* (New American Nation Series.) New York: Harper and Row. 1979. Pp. xvi, 384. $15.00.

With *The Confederate Nation* Emory M. Thomas has produced the best one-volume history of the Southern Confederacy. It incorporates the most recent scholarship and presents the story of the Confederacy vividly and with an eye for detail and human drama. Unfortunately though, for all its authoritativeness and vigor, this volume in the New American Nation series suffers from the demands made on its contributors for all-inclusiveness and breadth of coverage at the expense of originality of subject matter and of interpretation. Like previous general historians of the Confederacy, Thomas focuses on the military and ideological rather than the social and economic, leaving still peripheral the social history of the Confederacy. Moreover, by being forced to reiterate the familiar and dominant features of the Confederate struggle, the author has been prevented from exploring in depth the brilliant insights he revealed earlier in his provocative essay, *The Confederacy as a Revolutionary Experience.*

Although the story is then essentially familiar in its outline and main features, it has nevertheless been given a new interpretative cast by the exciting scholarship on the antebellum South which flourished in the 1960s and 1970s. In this debate, which is so central to the explanation of why the South seceded and formed the Confederacy, it is clear that Thomas shares Eugene Genovese's view that the economy of the old South was precapitalist and that separation was logical and necessary. But the cultural distinctiveness of the South was unformed and primarily negative in thrust, so that, once separation was achieved, a whole host of problems erupted relating to sectional identity and the purpose of independence. To secure their nation's autonomy, Southerners had increasingly to give up cherished values and practices. In their effort to be rid of the North, they had to adopt ways that were not considered intrinsically Southern. Centralization and government intervention began to supercede localism and individuality, manufacturing became all-important, and cotton proved to be less than regal. Hierarchy and class-rule came under assault, and even the enlistment and emancipation of the slaves became a matter of serious consideration. The nationalism demanded by the Confederacy after 1862 was a direct challenge to the revolutionary Southernism that was thought to be the essence of the revolt of 1860–61.

This crisis of identity, present at the Confederacy's birth and throughout its existence, is what Thomas believes gives meaning to and makes sense of the Confederate experience. Though this continuing problem provides the interpretative theme of the book, it is not clear whether this indecision about the ultimate identity and meaning of the Confederate nation is what explains the failure of the Confederacy. For Thomas never directly confronts this basic question. He does suggest that the Confederacy suffered a disadvantage in having not only to administer a fighting force but also actually to find and create one from scratch. He also comments that the long duration of the war presented a serious problem to an unprepared and overly sanguine South.

Neither of these observations, however, provides a sufficient explanation. The failure of the South to decide on its own identity and its consequent uncertainty of purpose is, by contrast, an intriguing and probably a rewarding line of inquiry. So it is a shame that, in this thorough and imaginative book, the insight is not fully developed. But then the reader of *The Confederate Nation* is too often left wishing that the book were an original and probing analysis of the life of the Confederacy rather than yet another, even though very good, interpretative overview.

MICHAEL PERMAN
University of Illinois, Chicago Circle

PETER MASLOWSKI. *Treason Must Be Made Odious: Military Occupation and Wartime Reconstruction in Nashville, Tennessee, 1862–65.* (KTO Studies in American History.) Millwood, N.Y.: KTO Press. 1978. Pp. xviii, 173. $18.95.

It is Peter Maslowski's contention that Reconstruction in the South began in wartime with the advent of Federal military occupation. In the case of Nashville, Tennessee, this came early in 1862. Within two weeks of the city's capture in February, Presi-

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dent Lincoln appointed Andrew Johnson military governor with the job of reestablishing loyal state and local governments. Henceforth, much Reconstruction policy was determined on the spot and not in Washington. Maslowski takes his title from Johnson's oft-quoted remark in August 1862: "Treason must be made odious, traitors punished and impoverished" (p. 80). The stern measures he soon employed selectively against Confederate sympathizers—disfranchisement, removal from office, banishment, imprisonment, monetary assessment, confiscation of property—antagonized not only the pro-Confederate majority of whites in middle and west Tennessee (who were already hostile) but also conservative Unionists who preferred conciliation. The ensuing conflict between these groups on one side and the minority of radical or unconditional Unionists, based in east Tennessee, was to characterize Tennessee Reconstruction both during and after the war, under Johnson as under his like-minded successor William G. Brownlow. Only after Johnson's sudden elevation to the presidency in 1865 did he veer toward the moderates and find himself in bitter conflict with the consistently radical Brownlow.

Wartime Reconstruction in Tennessee was a partial failure, Maslowski says; although Johnson set up a functioning civil government, it never became popular. Most Tennesseans remained loyal to the Confederacy, whose armies seemed capable of offering deliverance until the final winter of the war. The city government Johnson established in Nashville likewise received only grudging obedience and little support. Such municipal functions as refuse removal and enforcing smallpox inoculation had to be performed by the army.

Much of Maslowski's book relates to Tennessee rather than to Nashville. In some measure this is unavoidable, but the city is relatively neglected nonetheless. We get too little of Nashville's physical contours and what made it run in 1861 and too little systematic account of the changes that occurred subsequently. The book is organized topically, with separate short chapters devoted to civil-military relations, evolving Reconstruction policies, municipal government, and the treatment of blacks. A few colorful episodes are recounted, like the army's unhappy effort to banish a steamboat full of Nashville prostitutes to an unreceptive North; later the army adopted a highly successful policy of licensing, medical inspection, and treatment that nearly eliminated venereal disease in the city. But too many day-to-day events in the city's occupation history are lacking. One misses the breadth and felicity of Emory Thomas's account of wartime Richmond or Gerald Capers's of occupied New Orleans—richer subjects, to be sure. Nevertheless, it is a sound factual and interpretive account of Andrew Johnson's rule in Nashville and a worthwhile contribution to the history of Reconstruction in Tennessee.

ALLEN W. TRELEASE
University of North Carolina, Greensboro

JONATHAN M. WIENER. *Social Origins of the New South: Alabama, 1860–1885.* Baton Rouge: Louisiana State University Press. 1978. Pp. xiii, 247. $14.95.

In this broad, ambitious, and important book, Jonathan M. Wiener argues that the black belt planter elite reconstructed its hegemonic class position after the Civil War by shepherding its wealth despite the death and destruction of war, forcing black labor into a new oppressed state, employing political power to beat back a challenge by crossroads merchants, forming an opportunistic alliance with Yankee financiers to prevent the rise of an industrialist class in Birmingham until that potential Southern bourgeoisie in effect agreed to subject itself to planter domination, and spawning a "nonbourgeois agrarian ideology" (p. 187) to justify planter rule. Thus, the South was condemned, in the term of Wiener's thesis director, Barrington Moore, Jr., to follow "the Prussian Road" to modernization. Interesting and well-written, the book is a forceful statement of an original and coherent thesis. Yet I found it unconvincing at certain crucial points.

From a sample of individual United States census records for five Alabama black belt counties from 1850 through 1870, Wiener calculates the rates at which families continued to rank among the richest landholders in each decade, concluding that the 1860–70 rich planter persistence rate (43 percent) was comparable to the 1850–60 rate (47 percent). His methods may be questioned on two counts. First, the white population of these five counties decreased by nearly 16 percent from 1850 to 1870, yet his sample of rich planters in each year, 236, is the same. The result is that a white male adult had about one chance in thirty of being included in the planter elite, as Wiener defines it, in 1850, but one in twenty-five in 1870. He has thus biased his technique toward finding increasingly greater persistence. Second, he charts not individual, but family, persistence and does not change his procedure for calculating persistence rates to take this into account. "If two sons or both a planter and a son appeared in a subsequent list," Wiener informs us (p. 236), "that was counted as two cases of persistence." Again, this decision inflates his persistence rates, for planters' sons who inherited sufficient property are counted in the numerator, but not the denominator. Sons—or daughters—who moved away, changed names, died, or inherited insufficient property just disappear from the equation.

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Wiener contends that the planters, faced with a "labor shortage" because freedmen refused to work like slaves, created a "coercive mode of labor control" which "bound" black sharecroppers through legal as well as violent restrictions on the free labor market (p. 70). However, he presents almost no evidence that the laws were actually enforced, that the planters could be effective monopsonists for long, that croppers could not escape exploitative employers by moving on (stealthily if need be), that the income the blacks received was less than their marginal product, or even that blacks persisted in the same area long enough to make his non-free market contention plausible.

The 1866 and 1867 Alabama crop lien laws gave merchants and landlords equal rights to seize a tenant's crops to settle a debt. In 1871 the legislature gave landlords superior rights, and in 1885 and 1887 the legislature outlawed merchant crop liens entirely in half of the black belt counties but allowed their continuation in most counties of the state. Without closely analyzing the genesis of these acts or the nature of the coalitions favoring and opposing them, Wiener paints a picture of merchant challenge and devastating planter response which drove independent storekeepers out of the black belt and into the hill country, there to supply the white yeomanry. Yet it is difficult to see that the merchant challenge was all that grave: in the one black belt county for which Wiener gives complete figures, merchants owned but 2.6 percent of the real estate value in 1870, while that county's "planter elite" owned 63 percent (pp. 112–13). Moreover, he does not differentiate between city, town, and crossroads merchants, whose interests no doubt often clashed. And since adequate data from tax records or the 1890 individual census schedules do not exist, Wiener cannot test for the effect of the 1885 law, which best illustrates his merchant-planter conflict thesis, by tracing economic conditions of merchants in the eighties. While it seems obvious that the 1885 law must have discouraged black belt merchants, it is less clear why the 1871 law should have had such an impact, and Wiener's inference that the 1871 law caused the variations he traces in merchant persistence in the two areas in the seventies appears shaky.

In C. Vann Woodward's "New South," capitalists co-opted planters and donned nostalgic memories like sheeps' clothing. In Wiener's, planters forestalled "a bourgeois revolution" (p. 137) and evoked the Lost Cause to justify their continued rule and combat proponents of the "New South Creed." But Wiener's evidence is compatible with the view that the planter-industrialist and Montgomery-Mobile versus Birmingham clashes were between capitalistic, profit-maximizing interest groups, rather than socially competitive classes. And, though he makes some interesting criticisms of the "New South Creed" hypothesis, the refurbished Old South creed that Wiener extracts from newspapers, speeches, and pamphlets seems too disorganized and fragmentary to have served as a full-blown instrument of class hegemony after the war.

Despite its flaws, Wiener's book should focus controversy, stimulate research, and enjoy a wide readership.

J. MORGAN KOUSSER
California Institute of Technology

MELTON ALONZA MCLAURIN. *The Knights of Labor in the South.* (Contributions in Labor History, number 4.) Westport, Conn.: Greenwood Press. 1978. Pp. 232. $16.95.

The "New South" of the late nineteenth century posed formidable obstacles to unionization: the rural dispersal of its factories, mines, and timber camps, the poverty of its tenant farmers, and the systematic subordination of blacks were barriers to organization that appeared insurmountable. Nevertheless, Melton Alonza McLaurin argues, the Knights of Labor made themselves an effective economic and political force in every southern state but South Carolina during the 1880s. His study focuses on Virginia, North and South Carolina, Georgia, Florida, and Mississippi, with some discussion of strikes elsewhere. It is based primarily on the papers of Terence V. Powderly and John Hayes, official journals of the Order, friendly southern newspapers, and computerized data in the Knights of Labor Data Bank. It makes no effort, however, to supplement the story with knowledge of the Southwest, which has already been provided by Ruth Allen's two pioneering works, *The Great Southwest Strike* and *Chapters in the History of Organized Labor in Texas,* and by Roger W. Shugg, *Origins of Class Struggle in Louisiana.*

In McLaurin's view, the Knights grew slowly in the South before 1883, recruiting some urban craftsmen but enjoying the greatest strength among coal and iron miners in Alabama. Its major growth began in 1885, in the midst of a wave of strikes, the main characteristic of which was their aggressive quality: supposedly docile southern workers were demanding pay increases. Many of these strikes were successful, primarily because they caught the employers by surprise. Beginning with the 1886 lockout of textile workers in Augusta, Georgia, however, employers in one industry after another mounted devastative counterattacks. Alabama's coal miners were defeated and then reorganized outside of the Knights. Louisiana's black sugar cutters were savagely crushed by armed force. Black strikers in Mississippi's timber camps were replaced

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by whites. The mechanization of cigarette production eliminated the hand rollers, who had been the Order's strongest supporters.

Political ventures of the Knights experienced a similar pattern of momentary triumph followed by total defeat. McLaurin's account of the electoral coalition of blacks, Republicans, and labor reformers in Jacksonville, Florida, is most impressive: having captured the city's leading offices, the reformers watched helplessly as the state legislature stripped their municipality of self-government. Such campaigns gave special prominence to aspiring politicians, small businessmen, and journalists in the southern leadership of the Order and thus militated against the formulation of distinctly working-class demands or strategies. They also provided an important occasion for the Knights' experiments in interracial solidarity, a subject on which this book provides the best short discussion yet available. As economic and political defeats stripped the Order of its urban white membership, McLaurin argues, rural blacks continued to join, leaving the Knights "a fraternal order for the region's economic outcasts" (pp. 188–89).

The Knights of Labor in the South is a badly needed introduction to an important subject. McLaurin has disposed of the myth of the southern workers' "docility," but perhaps there is another myth that hobbles his attempts to appreciate this epoch: Gerald Grob's conception of "reform unionism" as an attempt by workers to "return the laborers to the proprietary status they had enjoyed before industrialization" (p. 39). McLaurin embraces this notion wholeheartedly, but it may be singularly inappropriate for the South, where the Knights' press coupled advocacy of industrial cooperatives and protective legislation with enthusiasm for local industrial growth and where state federations of labor in the next century were to reproduce the Knights' political demands almost verbatim.

DAVID MONTGOMERY
Yale University

H. WAYNE MORGAN. *New Muses: Art in American Culture, 1865–1920.* Norman: University of Oklahoma Press. 1978. Pp. xiv, 232. $14.95.

In *New Muses*, H. Wayne Morgan describes the "ideals and promises" of the American art world during an interesting and relatively unexplored period of American cultural history. More a summary than analysis, *New Muses* contains an enormous amount of bibliographical materials tidily organized according to the many and frequently conflicting ideas governing or relating to art from 1865 to 1920: attitudes toward art, the development of art interests through institutions and collecting practices, artistic life styles, the development of public art criticism, and new definitions of the artist's status and social role. This short and generalized essay ends with an examination of the coming of Impressionism and Modernism to the United States and their reception by artists, critics, and the public at large.

Invaluable for the materials it brings together, *New Muses* suffers from the author's approach. In organizing his subject thematically, Morgan misses a sense of development or change. As a result, he would have us believe, for instance, that when critics discussed their job in 1866, 1879, and 1907 they meant the same thing, even though forty years intervene between the first and last citation.

Morgan's method is germane to his thesis, which seems to be that the period possessed a continuity and unity despite its "basic tendency" in the direction of "innovation rather than elaboration of inherited methods and aims" (p. 145). Even Impressionism in this country remained "wedded to a realism based on studio conventions" and a concern for structure and form (p. 122). And Modernism, although based upon a changed definition of reality, emanated from the same concern with depicting nature as that which motivated earlier nineteenth-century Realists and the later Impressionists. "Realism," Morgan writes, "was the broad intent of the era's major styles" (p. 181).

Perhaps. But it should also be pointed out that Modernism developed out of a new, and therefore quite revolutionary, interest in formal elements in art that had nothing to do with either nature's reality or the reality of the artist's moods or emotions and from a new conception of the role of art that had been growing alongside Impressionism during the last decades of the nineteenth century. The new "art for art's sake" philosophy that was so brashly bruited by the Pre-Raphaelites and proponents of the Decorative Movement and that entered into the taste for the Japanese and the Renaissance and into the new collecting habits of turn-of-the-century patrons, fed into the excitement over Post-Impressionism that eventuated in the development of modern art. It should be said that Morgan suggests something about the revolutionary nature of Modernism, but he seems confused about its impact on the American tradition in art, at one point suggesting that it merely "fractured" existing unity and, at another, indicating that it constituted a major change (pp. 180, 181).

Some of the confusion in Morgan's otherwise invaluable study emanates from his concentration on the literature of art criticism rather than on the art works themselves and from his effort to impose unity on an art world that in fact fell into two parts. Up to around 1890, one can see large areas of consistency in what Americans thought and did with

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respect to art. From around 1890 to 1920, however, a marked change in the kind of art produced and in museums' and collectors' attitudes toward art occurred, auguring a revolutionary change that Morgan rightly sees would eventually bring about a separation of American culture into elite and mass tastes and would, indeed, "affect all future American art" (p. 182).

LILLIAN B. MILLER
National Portrait Gallery

LEONARD DINNERSTEIN *et al. Natives and Strangers: Ethnic Groups and the Building of America.* New York: Oxford University Press. 1979. Pp. xiv, 333. Cloth $13.95, paper $4.00

This collaborative volume surveys chronologically the "role" of ethnic minorities in the evolution of modern America since the seventeenth century. Defining an ethnic minority as "a group with a shared or common culture and sense of identity based on religion, race or nationality" (p. ix), the volume's ten chapters concentrate primarily on the experiences of blacks, Indians, and immigrants. They also show the interaction of these minorities with a white majority created by people of English descent, who eventually assimilated descendants of some other Europeans. The volume includes a nine-page bibliography of secondary literature and over fifty maps, illustrations, and statistical tables. The work, however, dispenses with footnotes and cites no primary sources.

The labor performed by ethnic minorities in the growth of the American economy is emphasized by the authors, who think that "most standard histories of the United States" have not paid adequate attention to it (p. viii). Thus the volume outlines a record of an ethnically diverse population engaged in different kinds of agricultural and industrial labor. Its outline begins with English settlers recruiting indentured servants and black slaves to augment the limited supply of Indian labor. It next reviews the place of slavery in the southern economy and the influx of European immigrants into the northern and western economies before the Civil War. As millions of East and South Europeans entered the burgeoning industrial system between the 1860s and 1920s, Indians were isolated on reservations, Chinese were recruited for building railroads, and blacks were moving to northern cities. The outline ends with the period after World War II recounting the importance of Mexican-American farm workers, the arrival of immigrant refugees, and the migration of Puerto Ricans to the mainland. Although these minority workers usually received meager rewards for their labor, the descendants of immigrants experienced more upward economic mobility in recent times while the Indians, blacks, and Spanish-speaking population still lagged behind.

These minorities expressed group consciousness in various ways. Often they published newspapers and organized churches, mutual-benefit societies, and other associations. Also, immigrant organizations cultivated awareness of their nationality and tried to instill it in their children, who could not easily resist the process of assimilation. Yet the descendants of some minority groups, like their ancestors, did not easily assimilate and had "second-class" status partly because of the barriers created by "old stock" American nativists and racists. By the 1970s such barriers were somewhat lessening, while ethnic self-consciousness became stronger among descendants of immigrant minorities.

Although emphasizing the economic activities more than the other experiences of the minorities, the volume does not always integrate them effectively. It devotes considerable space to nativist and racist responses to minorities and only briefly links these factors to economic considerations such as competition for jobs. Also, some chapters that review different economic developments and individual group experiences lack thematic unity, since the material is arranged under separate subheadings with only sketchy and infrequent comparisons and contrasts. Nevertheless, the volume brings together a great range of information while highlighting the economic contributions of minorities. In short, its intended users—students and general readers—will find it a useful introduction to "grass roots history."

A. WILLIAM HOGLUND
University of Connecticut

CARL REEVE and ANN BARTON REEVE. *James Connolly and the United States: The Road to the 1916 Irish Rebellion.* (AIMS Historical Series, number 10.) Atlantic Highlands, N.J.: Humanities Press. 1978. Pp. xvi, 307. $16.00.

James Connolly occupies a position of importance in the history of Ireland. Convinced that economic and political freedom were inseparable, he devoted his life to the struggle for both the independence of Ireland from Great Britain and the economic independence of the Irish people through socialism. As Ireland's leading Marxist, he was prominent in the Irish socialist and labor movements. His activities as Commander of the Dublin Garrison during the Easter Rising of 1916 resulted in his execution by a British firing squad.

These facts are well known. Less commonly known is the fact that Connolly spent the years from 1902 to 1910 in the United States where, the authors of this study contend, he was "forged and

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tempered" for the trials that awaited him in Ireland. It is on these years that Carl Reeve and Ann Barton Reeve focus their attention because, they believe, "Connolly's substantial theoretical and organizational contributions to the American working class movement are largely unknown in this country and have been almost completely ignored by American labor historians" (p. x).

Connolly first visited the United States in 1902 on a fund-raising tour arranged with the help of the Socialist Labor Party (SLP). He settled here in 1903 and became active in the SLP, the Industrial Workers of the World and, ultimately, the Socialist Party of America. He also organized the Irish Socialist Federation and edited its monthly publication, the *Harp*, from 1908 to 1910. Two of his works, *Socialism Made Easy* (1909) and *Labor in Irish History* (1910) were written during this period.

In spite of his long residence in this country, Connolly never liked either the United States or the American socialist movement. Although Daniel DeLeon initially attracted him here, Connolly was soon at odds with the SLP leader and in 1908 he abandoned the SLP for the SPA because of the dispute. As the Reeves tell the story, Connolly's American years were one long feud with DeLeon. The two men, neither of whom tolerated opposition, differed heatedly over a variety of topics from Marxist theory to the way the SLP was run. "There is but one supreme test of a man's soundness in the SLP. . . ," Connolly is quoted as saying. "The man must be prepared to burn incense at the Shrine of DeLeon . . ." (p. 98).

The Reeves have produced a book that is not without merits, yet ultimately it will disappoint the student of American radicalism. They have presented the facts of Connolly's American years, and the reader becomes fully informed about the disputes he engaged in while in this country. And, although the Marxism of the authors is apparent, it is not obtrusive. But the book falls short of promise. The whole person does not emerge as the Reeves hoped he would, and it is unfortunate that they set this as a goal when it was not necessary in so limited a study. Equally unfortunate, neither writing style nor organizational ability is a strength of the authors. But the most serious defect of the book is its failure to persuade the reader that Connolly's long American visit was important to American radicalism. If James Connolly made the significant contributions that the Reeves claim, we need a more convincing study to drive this significance home.

HAROLD W. CURRIE
Michigan State University

NORMAN L. CROCKETT. *The Black Towns.* Lawrence: Regents Press of Kansas. 1979. Pp. xv, 244. $14.00.

Others have written on individual black communities and on the "Black Exodusters" of 1879, but this is the first book-length treatment of all-black towns. Norman L. Crockett focuses on the settlement, growth, and decline of five of these communities—Nicodemus, Kansas; Mound Bayou, Mississippi; and Langston, Clearview, and Boley, Oklahoma. At least sixty all-black towns were founded between 1865 and 1915, but records of many of them are almost nonexistent. Crockett regards these five as representative.

Ideologically, "self-segregation inside the United States within the protected confines of an all-black community" represented one form of blacks' continuing "quest for a secure position in the American system," a way of being "both black and American" (pp. xi, xii). Promotional literature stressed escape from white prejudice and oppression and the opportunity to prove the ability of blacks to manage their own affairs. Community leaders subscribed to the philosophy of Booker T. Washington. They believed in the capitalistic system and hoped that blacks would ultimately be incorporated into the mainstream of American society.

Economic motives were mixed with ideological, and the story of these towns is also part of the larger story of the American frontier, an aspect that perhaps deserves more attention than the author gives it. Railroads, public land, and promoters were involved in establishing the towns, as were some remarkable black leaders, such as Edward Preston McCabe, Isaac Montgomery, Charles Banks, and James E. Thompson.

Perhaps the most illuminating feature of Crockett's account is its insight into racial attitudes and race relations. Within the limits of the towns, blacks enjoyed autonomy and independence, but they could not be totally isolated from the outside world. The existence of the towns aroused the fears of whites and intensified racial antagonisms. Residents of black towns were highly politicized. They of course elected their own mayors and town councils and managed internal affairs. But whites generally shunned blacks who attempted to participate in county politics in Oklahoma as well as in Mississippi. At the state level, Oklahoma blacks were almost powerless politically even before the adoption of the famous Grandfather clause. Many Oklahoma towns barred blacks from their borders except during daylight hours.

In Oklahoma, race relations were further complicated by the presence of an Indian population and black freedmen who had been slaves of Indians. Some Indian tribes were unwilling to include their freedmen in tribal membership. On the other hand, some former slaves of Indians sought a higher status than that of newly arrived blacks from the South. But whites refused to recognize these distinctions,

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and increasingly those Indian tribes that had been more tolerant accepted white standards and moved toward total segregation of blacks, including persons of mixed Indian and African ancestry.

EMMA LOU THORNBROUGH
Butler University

JUDY JOLLEY MOHRAZ. *The Separate Problem: Case Studies of Black Education in the North, 1900–1930.* (Contributions in Afro-American and African Studies, number 42.) Westport, Conn.: Greenwood Press. 1979. Pp. xvi, 165. $14.95.

Judy Jolley Mohraz wrote *The Separate Problem* to give "historical perspective to . . . the contemporary crisis in urban education" (p. xvi). To provide this perspective, Mohraz presented case studies of black students in the public schools of Chicago, Indianapolis, and Philadelphia from 1900 to 1930.

Both her end and her means present theoretical and organizational problems. Historical studies written explicitly to explain the present risk subordinating past issues and developments to current concerns. Case studies contribute little to an understanding of a general topic unless they exemplify its basic elements and are carefully related to existing studies of the subject. Mohraz's book does not deal adequately with either of these problems.

Mohraz argues that several educational "reforms" of the early twentieth century—intelligence testing, ability grouping, vocational education, and guidance counseling—helped preserve a racist status quo by discriminating against black students. Her chapters on these educational practices are, primarily, familiar polemics against them rather than case studies of the practices or of black reaction to them. Indeed, Mohraz admits that "'most Negroes in the three cities remained silent about the use of intelligence tests" and that the resulting "ability groupings . . . did not arouse great controversy within the black community" (pp. 58–59). Black leaders in the three cities agreed that black students should not be limited to vocational education, but they supported its inclusion in the public schools. While many guidance counselors probably did steer black students into vocational curricula or black colleges, Mohraz does not cite even an assertion that this occurred in these cities before 1930.

The book's remaining chapters describe race relations, the environment of black children, and the development of segregated schools in the three cities. Mohraz's treatment of the first two topics is a brief (thirty-seven pages) survey based almost entirely upon secondary sources. Her case studies of the rise of segregated schools, before and after the Great Migration, however, are important contributions to an understanding of education in the three cities.

The extent to which these case studies of segregation help explain "Black Education in the North, 1900–1930," however, is problematic. Mohraz never explains her criteria for choosing these cities, nor does she systematically relate her work to several existing studies of blacks in other northern cities or in northern or southern states from 1900 to 1930.

Like the schools it describes, this book has "separate problems"—problems of insufficient separation of the present from the past and scholarship from polemic, along with the problem of excessive separation of this study from other books that deal with segregation or education.

ROBERT G. SHERER
Wiley College

LEONARD J. ARRINGTON and DAVIS BITTON. *The Mormon Experience: A History of the Latter-Day Saints.* New York: Alfred A. Knopf. 1979. Pp. xiv, 404. $15.00.

This work by two distinguished and committed Mormon historians is an attempt to encompass the "Mormon experience" in a volume that is "analytical, interpretative, topical history" (p. xii). Its primary appeal is to the general reader. Heavily dependent on secondary sources, it seeks to "understand as scholars of any faith or no faith would seek to understand." Yet the authors maintain that their "empathy" gives them special insight, that "some matters can never be understood adequately except from within" (p. xiii).

In many ways they succeed. In a lively style they trace Mormon history since 1830, seeing the appeal of early Mormonism in its Christian primitivism, its modern revelation to settle sectarian issues, its millennialism, and its mythic potency. Depicting the central Mormon concern as building a theocratic kingdom, they indicate the recurring conflicts this engendered with non-Mormons. Driven west, the Mormons found no refuge, for there they encountered federal authority and were forced to make major accommodations to American culture. Still, the church maintained its identity and vigor, even through a period of twentieth-century challenges, including urbanism, secularism, and increasing involvement in the outside world. The best chapters deal with the Mormon character, which the authors see as pragmatic, perfectionist, cooperative, antispecialistic leading to superficiality, pessimistic toward the world, authoritarian, and oversensitive to criticism.

Having a composite authorship and a topical arrangement, the book is often repetitive, dealing with topics like the Book of Mormon, Brigham Young, the exodus and polygamy in several different places (pp. 13–15, 31, 82–96, 179–81, 194–205, 230). This is most noticeable in the final pages of

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part two and the beginning of part three. Not only is the twentieth century dealt with in two separate but adjoining chapters, but the introduction to part three also repeats material discussed earlier (pp. 231–42).

The authors at times demonstrate a strong Mormon bias that leads to errors that may not be observed except by specialists. They affirm that all the allegations of bad character against the Smith family came from E. D. Howe (p. 10); that all the Book of Mormon witnesses remained true to their testimony (ignoring Oliver Cowdery's renunciation acknowledged by the *Times and Seasons* and Martin Harris's conversion to Shakerism, which he said he believed in more than he ever did in the Book of Mormon [p. 14]); that there were no tensions in the Smith family sufficient to bother young Joseph (p. 18); that the Book of Mormon has the internal complexity of the Bible, (ignoring the romantic disposition of its plot and central characters [p. 33]); that the Carthage Greys stormed the jail (p. 59); that polygamy started in Nauvoo (pp. 69, 196–97); that the doctrine of graded salvation was introduced at Nauvoo (p. 196); that, contrary to recent findings, the Mormons treated the Indians more humanely than others (pp. 145–60); and that some important archeological evidence tends to support the Book of Mormon story of Indian origins, although the authors are no doubt aware that the view of most scholars is strongly to the contrary—that American Indian culture tends overwhelmingly to be autochthonous (p. 15).

The booster spirit of the book is particularly evident in the later chapters, which gloss over tensions and detail materialistic achievements (pp. 262–335). This is also evident when the authors inform us that 20 to 30 percent of the Mormons in Utah are inactive or marginally so and that over half elsewhere are. (p. 288). They do not tell us why or what it means to be a Mormon dissenter. A "Mormon" is defined as one who belongs to the Utah church and is active, although the authors admit Mormonism constitutes a subculture (p. xi). The affirmative bias is conspicuous in that nothing is said about the origin of the Book of Abraham, one of the major issues among scholars in recent years.

More troublesome, perhaps, is the antihistorical tendency throughout to trace events and depict Mormon character without demonstrating causal interrelationships. Thereby the authors fail to make clear why the events they describe are critical in understanding Mormons and Mormonism.

Still, the book is a commendable job, a far advance beyond previous works by incumbent Mormons. In every chapter, usually parenthetically, they acknowledge tensions, limitations, errors, and faults. They are probably right that the adjustments made in the twentieth century have kept a majority content and devoted to the faith, so that their volume is reflective of these Mormon attitudes as well as descriptive.

MARVIN S. HILL
Brigham Young University

WILLIAM S. BERLIN. *On the Edge of Politics: The Roots of Jewish Political Thought in America.* (Contributions in Political Science, number 14.) Westport, Conn.: Greenwood Press. 1979. Pp. 206. $18.95.

It has long been an acceptable device for historians to depict and generalize a period from the works of writers in that era. The danger in this stratagem lies in determining how representative any given person is of the time in which he or she lives. The works of a Melville or an Emerson surely tells us much about nineteenth-century America; we would, and rightly so, be suspect of generalizations built on the writings of Dion Boucicault.

In essence, this is the problem with William Berlin's *On the Edge of Politics*, a study which attempts to delineate the roots of American Jewish political thought. Berlin's thesis is that Jews, because of their insecure position in society, have traditionally distrusted politics, yet at the same time have been attracted to it. They thus stand at the "edge," fascinated by power yet fearful of corruption. It is an interesting notion and in some limited spheres even contains elements of truth. But Berlin's book is shot through with methodological problems and, in some instances, with historical inaccuracies as well.

He starts with a chapter on Louis D. Brandeis, whom he considers "the most prominent Jew in American political life" in 1914. Berlin then goes on to analyze Brandeis's political ideas as reflective of the German-Jewish elite. If anything, Brandeis was far removed from the social as well as the political circles of the *yahudim*; if Berlin had wanted a representative of "our crowd," he would have done better looking at the ideas of Louis Marshall or Oscar Strauss. Berlin's reading of Brandeis's political views also seems confused; he did not mistrust politics, as Berlin says, but was suspicious of the urban political machines, and in this he was not so much the Jew as the progressive reformer.

The bulk of the book is devoted to the ideas of several Yiddish writers, whom Berlin claims accurately reflect the views of the eastern European immigrant masses. But Berlin admits that his choices were prompted in large part by the availability of Yiddish works in English translation. Thus he analyzes the English writings of Abraham Cahan, longtime editor of the influential Yiddish daily, *The Forwards*. There is no doubt that Cahan was one of the most important writers of his time, but the place to look for his ideas is in the editorials he wrote for nearly four decades, all of which are in Yiddish.

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Similarly, many important Yiddish writers are ignored because of the unavailability of their works in English. Isaac Bashevis Singer, on the other hand, receives extensive analysis even though he did not arrive in this country until after the heyday of Yiddish culture and despite the fact that his works have been so well received not for their political ideas but for their naive yet sophisticated portraits of the Old World. Berlin also deals with two minor writers, Samuel Orintz and Marcus Ravage, primarily, it would appear, because their novels had been translated or written in English.

In his brief bibliographical essay, Berlin writes that "the role of Jews in American political life deserves more attention than it has received." This is true, but unfortunately books like this will not further that goal.

MELVIN I. UROFSKY
Virginia Commonwealth University

STEVEN HERTZBERG. *Strangers within the Gate City: The Jews of Atlanta, 1845–1915.* Philadelphia: Jewish Publication Society of America. 1978. Pp. xiii, 325. $12.00.

In an age when national attention is focused on Carter country, historians have shown an avid interest in unraveling the mystique surrounding the Old Confederacy as well as in heralding the emergence of the New South. The recent establishment of the Southern Jewish Historical Society is an indication of the appearance of professionalism within a field too often reserved for the dilettante.

An exponent of the "new social history," Steven Hertzberg has utilized sophisticated scientific techniques to describe the development of a major southern Jewish community while attempting to establish a model for future local histories. His study encompasses the inception of the Jewish community of Atlanta in 1845 until its evolution by 1915 into the South's third largest Jewish community. *Strangers within the Gate City* includes sections on institutional growth, the impact of immigration, and the relationship between Jews and blacks. The much-discussed Leo Frank case (1913–15), wherein a Jewish factory superintendent was lynched in an atmosphere of burgeoning anti-Semitism, concludes his treatment of Atlanta Jewry.

What is unique about Hertzberg's volume is that information obtained on almost every Jewish inhabitant of Atlanta in 1870, 1880, and 1896 serves to explain hitherto neglected areas of communal history such as residence patterns, as well as geographic and social mobility. Within the Atlanta Jewish community, the author has discovered a greater degree of upward social mobility and a lesser extent of geographic mobility than among gentile neighbors. Among southern Jewish communities today, Atlanta Jewry is exceeded in population only by the Jews of Miami, Houston, and Dallas.

An expansion of the subject until the contemporary era would have been highly desirable. The author is to be applauded, however, for his pathfinder endeavor. Hopefully, his seminal study will encourage other in-depth histories of local Jewish communities so that, eventually, a definitive picture of the entire gamut of the American Jewish experience can be written.

However justifiable is the author's critique of the existing literature in the field of American Jewish history, he underestimates the contribution of many nonprofessional, filiopietistic predecessors who admittedly have provided us with "romantic" conceptions of the past. At times, the grandiose generalizations of an "outdated" but classical historian such as Heinrich Gratz or the insight of writers who were witnesses to events create a more realistic portrait of events than some detailed depictions of more contemporary authors. In fact, while the author's study of Atlanta Jewry constitutes a historian's delight, it is not necessarily grist for the general reader's mill.

Within his sphere of investigation, the author has no peer but, venturing upon the ocean of generalization based upon secondary sources, he encounters stormy seas. For example, Hertzberg's estimate that three thousand Jewish soldiers served in the Confederacy is not indicated by his source, Bertram Korn in *American Jews and the Civil War*, nor by Simon Wolf's, *The American Jew as Patriot, Soldier and Citizen*, upon which the Korn citation is based. His assertion that the existence of slavery in the South was a major factor in impeding Jewish immigration is also questionable, especially in view of the fact that most southern Jews, whether native or immigrant, usually mirrored the proslavery attitude of their Christian compatriots. Economic and geographic considerations were probably more effective deterrents to Jewish immigration to the South than the slavery issue. Hertzberg also contends that slavery and the Negro question were sociological safety valves, insulating the southern Jew against prejudice—an oft-mentioned assumption. But, perhaps it was the frontier nature of southern Jewish communities and the fact that they were assimilated and well-integrated that accounts for the muting of anti-Jewish feeling in this area.

Nevertheless, the reader will find *Strangers within the Gate City* to be both a provocative and incisive analysis of an important southern Jewish community. Despite the author's occasional tendency to draw too many far-reaching, unsubstantiated conclusions on topics beyond the range of his investigation, his work is a new departure in local historical

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writing—an example that others would do well to emulate.

MYRON BERMAN
Temple Beth-El,
Richmond, Virginia

FRANCES KRALJIC. *Croatian Migration to and from the United States, 1900–1914.* Palo Alto: Ragusan Press. 1978. Pp. vii, 113. $8.00.

This volume by Frances Kraljic, an outgrowth of her Ph.D. dissertation, is a most valuable contribution to the field of immigration history. Kraljic's monograph has such importance because it focuses on a little-examined aspect of the immigrant experience in the United States, namely voluntary repatriation. During each of the great migrations to this country, there have been substantial numbers of immigrants who have ultimately returned to their homelands. Yet, in the growing literature of immigrant and ethnic history, scant attention has been paid to the "returned emigrants," their motivations, and impact on the "Old Country." As Kraljic demonstrates, a mere handful of previous books has described repatriation, and none of these has dealt with an Eastern European nationality.

Kraljic begins with a brief discussion of Croatian peasant immigration to the U.S. during its peak years. She has an excellent command of all the available statistical sources, both Yugoslav and American, covering Croatian migration and clarifies many of the difficulties with these statistics. After describing the massive, economically motivated migration of young peasant males to American industrial cities, Kraljic begins to detail why *one-third* of all Croatian immigrants ultimately returned to Croatia. Many immigrants initially had come to the U.S. seeking "economic betterment" through a *temporary* work experience in the mine or mill. Accumulated savings led immigrants to return to Croatia to buy additional farm land and improved housing. In addition, unemployment during recessionary periods prompted many to repatriate.

Once returned, these "Amerikanci" could not fail to have impact on the "Old Country" villages. The returnees deliberately emphasized their distinction from peasants who had not left Croatia. Returned emigrants used what few English words they knew in their conversational speech and tended to form friendship circles that excluded Croatians who had not lived in America. Repatriates dressed in American rather than traditional peasant clothing. They exhibited a marked improvement in the management of their farms. Many founded small businesses with the "capital" they had accumulated as factory workers and miners in the U.S. Repatriates brought American products like sewing machines and phonographs into Croatian villages. Most returned to their families, but as "different men," with attendant marital difficulties. All of this evoked mixed reactions from Croatians who had not made the trek to America. Returned emigrants were greeted with both admiration and jealous ridicule. Yet, as Kraljic notes, repatriates exerted great influence through "their financial situation, innovative spirit and new ideas" (p. 103).

Kraljic draws upon statistics and contemporary accounts for her evidence. She also includes one detailed individual case study, using the interview technique. More such portraits would have strengthened the book. Yet, in sum, this is a most enlightening, thorough study of an important and neglected area of immigrant history.

EDWARD A. ZIVICH
Calumet College

INGRID SEMMINGSEN. *Norway to America: A History of the Migration.* Translated by EINAR HAUGEN. Minneapolis: University of Minnesota Press. 1978. Pp. vii, 213. $12.95.

Norway to America, ably and imaginatively translated by Einar Haugen from the Norwegian original *Drøm og dåd* (*Dream and Deed* [1975]), is the first book-length presentation in English of a distinguished Norwegian scholar and professor of history. Ingrid Semmingsen introduced emigration research to Scandinavia with her two pioneering studies *Veien mot vest* (*The Way West* [1941 and 1950]), and the present volume represents the state of her research to date.

It is the work of a mature scholar who has achieved an intimate and perceptive understanding not only of the social history of her own country, but also of the unique dynamics of American society and of the circumstances—the "push" and the "pull"—that explain the great Norwegian migration to the United States in the nineteenth and twentieth centuries. Semmingsen provides an account of the movement rich in human drama and illustrative detail. Placing the narrative within a broad conceptual framework, and drawing on recent scholarship, she develops a convincing interpretation of the exodus from Norway and traces Norwegian settlement in the upper Midwest and on the Pacific coast. She discusses the emergence and the shifting fortunes of a Norwegian-American subculture, finally assessing current interest in ethnic origins and culture.

Semmingsen tends to support those economic historians who view migration causation in terms of the demographic situation and the differing economic conditions in Europe and America; she apparently accepts Thorvald Moe's conclusion that 87

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percent of emigration from Norway falls in this category. Other factors as well operated to produce an emigration intensity that surpassed that of almost all other European countries; the early appearance of immigrant communities created a self-generative effect and established a flow of information and a positive image of America in the minds of potential emigrants. Such circumstances, in addition to an improved transportation system, increased economic capability—including prepaid tickets—and the restlessness and the rising expectations growing out of dramatic changes and progress within Norway itself illuminate the driving forces and the course of the movement of people from Norway to America.

Norwegian-American ethnicity has shown great tenacity when measured by rate of intermarriage, language retention, and religious and organizational activity. Political and economic interests, as well as educational and professional opportunities, and the experience of World War I, moved the immigrants and their children toward assimilation and slowly dissolved immigrant communities. Although the lines of development are clearly stated, the story in this part of the book appears sketchy at some points: also, because of inconclusive evidence, there is no attempt at an exhaustive analysis of the social background of the emigrants. But, certainly, Semmingsen has produced a work of superior quality and merit, presenting in one brief volume a comprehensive treatment of Norwegian migration and settlement that will appeal with equal strength to the critical scholar and the interested layman alike.

ODD S. LOVOLL
St. Olaf College

PAOLO E. COLETTA *Admiral Bradley A. Fiske and the American Navy*. Lawrence: Regents Press of Kansas. 1979. Pp. xii:, 306. $25.00.

RONALD SPECTOR. *Admiral of the New Empire: The Life and Career of George Dewey*. Baton Rouge: Louisiana State University Press. 1974. Pp. xvi, 220.

These biographies of two prominent, early twentieth-century American naval figures invite comparison. Both admirals George Dewey and Bradley A. Fiske attained the peak of influence in their careers just prior to the entry of the United States into World War One, Dewey as president of the General Board of the Navy, and Fiske as aide for operations under Secretary Josephus Daniels.

Historians have largely neglected the great struggle between Fiske and Daniels over naval planning, organization, and preparedness. Paolo E. Coletta has taken up this issue with great care, though with an unmistakable preference for Fiske's side. Admiral Dewey received tremendous notoriety for his victory at Manila Bay in 1898, but has likewise attracted little serious historial attention, especially to his significant service with the General Board. Ronald Spector's biography carefully outlines Dewey's long tenure on the General Board and achieves a more detached perspective than Coletta, though neither author challenges the prevailing view of the two officers. Fiske remains, as in his autobiography, *From Midshipman to Rear Admiral* (1919), the beleaguered and unappreciated naval inventor, author, administrator, and prophet. Spector concludes that Dewey's bluntness with the press and political naiveté made him "simply not suited to the role of naval statesman in a world where the simple verities of nineteenth-century Vermont and Annapolis no longer applied" (p. 204).

It is easy to favor Bradley A. Fiske. His spare frame and frequently poor health never curtailed a productive career of impressive achievements. As an inventor alone his reputation would have been secure. His work with gunnery and fire control put him at the vanguard of technical developments in the period 1890–1910. And he immediately grasped the military potential of air power, including a call for the development of the torpedo plane. As an author, his six books and sixty-five articles place him second in productivity among active officers only to Alfred T. Mahan. As a desk officer or in command of ships of the new navy, Fiske consistently earned high praise for his abilities, though Coletta has too assiduously included summary remarks from almost every fitness report.

Fiske's most difficult battle was against the civilian government's unwillingness to engage in the naval planning, organizational reform, and preparedness program he and many military men believed essential for the security of the nation. The civil-military relationship is at the heart of the dispute, and Coletta's analysis would have been improved by reference to Peter Karsten's *The Naval Aristocracy: The Golden Age of Annapolis and the Emergence of Modern American Navalism* (1972) and Richard Challener's *Admirals, Generals, and American Foreign Policy, 1898–1914* (1973).

Aside from some small mistakes—President Theodore Roosevelt had not "just been elected President" in November, 1901 (p. 66) and the Hudson River is not the body of water that divides Manhattan and Brooklyn (p. 11)—the research on Fiske's life is extensive, the study thorough. Coletta carefully and honestly describes the secret connivance of Fiske and other officers with congressmen, journalists, and even a hired public relations firm in their struggle with President Wilson, Secretary Bryan, and Daniels.

Ronald Spector uses a wide range of sources and

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a bright, clear style to make his biography of Admiral Dewey immensely attractive. The Battle of Manila Bay, the conflict there with the Germans, and the fall of Manila are dramatically and sharply described, though little new is added to our understanding. The role of the state department and the intrigues of the American far eastern consuls all bore heavily on Dewey's outlook and actions in the Philippines, but they are not conclusively examined. Spector points out that there was nothing more "important and more tragic" than Dewey's relationship with the Filipino insurgents (p. 83), yet this too is incompletely developed. He is similarly insufficient on the relationship between Dewey and Aguinaldo, and I believe he erroneously attributes Dewey's caution on the question of the final disposition of the Philippine Islands to his temperament rather than to his political ambition. For the important four-month period from February to May, 1899, after the outbreak of the insurrection against the United States while Dewey served with General Elwell S. Otis on the Schurmann Commission, we have a scant two-page summary (pp. 99–100).

Spector carefully outlines Dewey's long tenure on the General Board, where the Admiral of the Navy emerged as statesmanlike and capable once the spotlight of publicity was withdrawn. In some contradiction to his own conclusion, Spector depicts Dewey's leadership of the board as forceful and even handed in the disputes over the distribution of the fleet, the proposed base at Subic Bay, the all big-gun battleship, and the movement toward a naval general staff.

PHILIP Y. NICHOLSON
Nassau Community College

EDMUND MORRIS. *The Rise of Theodore Roosevelt.* New York: Coward, McCann and Geoghegan. 1979. Pp. 886. $15.95.

Readers seeking a monograph packed with information about Theodore Roosevelt's pre-presidential years will not be disappointed in this thick book by Edmund Morris. Evidently the product of the author's long-standing interest in the life and times of Theodore Roosevelt, the book presents extensive treatments of every significant aspect, public and private, of these eventful years in the Rough Rider's life. Morris's research is thorough, and the quality of his writing is excellent. In particular, his word portraits of many leading politicians—Thomas Platt, Mark Hanna, and Thomas B. Reed, among others—are literary gems.

Morris's biography inevitably invites comparison with Carleton Putnam's volume on Roosevelt's formative years to 1886. Although Morris carries the story fifteen years further, with regard to interpretation he is at one with Putnam. Both authors emphasize the maturity, responsibility, and integrity of young Roosevelt's conduct. Little of the self-seeking perpetual adolescent, an image of T. R. popularized in Henry F. Pringle's classic account, surfaces here. Not that Morris is uncritical of Roosevelt; he scrupulously mentions T.R.'s egotism, bellicosity, and hunger for power. But the bulk of Morris's presentation is so admiring as to make these negative qualities seem minor. Morris has a tendency to idealize some subjects, as in his passages on T.R.'s parents, where he does not indicate how their influence might account for some of their son's less attractive qualities. Also, especially in the first half of the book, Morris too often presents T.R.'s opponents as Roosevelt himself saw them—wrong-headed, dishonest, self-seeking politicos. Roosevelt, by contrast, is portrayed as incorruptible, a public-spirited reformer. Legitimate reasons for opposing Roosevelt are not seriously explored.

Despite the massive amount of detail in Morris's book, one closes it with the feeling that some central element of T.R.'s character remains unexplained. Roosevelt was a study in contradictions. He loved the natural world but spent much of his life killing animals for sport. His style in dealing with many political problems was to make a frontal attack, but when faced with difficult situations in his personal life he often did just the opposite, refusing to confront them and severely repressing the memory of unpleasant events. He had an apparently genuine affection for many of his fellow men but was inordinantly eager to launch the war with Spain and to get to Cuba to kill somebody. In leaving these and other dichotomies in Roosevelt's personality largely unresolved, Morris has plenty of company. As long ago as 1961, Dewey W. Grantham, Jr., noted that Roosevelt's biographers had a tendency to love or loathe the man and to focus on the aspects of his personality most consistent with their views. It does not diminish the very real contribution of Morris and other biographers to suggest that a frustrating but essential task must still be done: that of trying to fit the contrasting elements of Roosevelt's personality into a portrait of the whole.

GERALD W. MCFARLAND
University of Massachusetts, Amherst

JOHN M. DOBSON. *America's Ascent: The United States Becomes a Great Power, 1880–1914.* DeKalb: Northern Illinois University Press. 1978. Pp. 251. $15.00.

This book deals with an interesting and controversial period, already much studied. Although a survey, it makes some pretensions to original scholarship. The depth of its penetration as a study of diplomacy, its principal emphasis, is all too tellingly

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indicated by the author's bibliographical comment on the official compendium *Papers Relating to the Foreign Relations of the United States*: "An examination of the documents in the files of the National Archives shows that those omitted from *Foreign Relations* add almost nothing to what can be derived from the published volumes" (pp. 229–30). To put it bluntly, if you believe that, you will believe anything. Actually John M. Dobson has based his book largely on secondary sources, with frequent quotations from *Foreign Relations* and other published documents and an occasional citation from the papers of the most important policy makers. The *Congressional Record* and some contemporary periodical articles appear but no newspapers.

There is certainly no objection to using good secondary accounts, whose authors have carefully examined primary materials, and Dobson has chosen some of the best. Unfortunately his basic thesis, like many of his materials, is derivative—that turn-of-the-century American expansionism sprang from economic, political, and moral sources. In a good introductory chapter he evaluates these sources along familiar lines. To his credit, Dobson resists strongly the economic quasideterminism (not his term) of recent "revisionist" diplomatic historians. Perhaps he resists a little too strongly, for he undervalues some economic-strategic matters, the isthmian problem, for example, and makes the distinction between economic and political aims even less clear than it appeared in real life. There is also considerable fuzziness in specifying and illustrating the "moral" factor. Was it ideological, humanitarian, or both? Dobson emphasizes the ideological and undervalues missionaries as a force in American foreign relations.

The superstructure above these ill-defined foundations contains many insecure "snap judgments." Two chapter titles (chapters 3 and 4) will have to suffice as illustrations. Did the United States "sidestep an empire" in the early 1890s and then "seize" it in the laft half of the decade? Perhaps, but only if one focuses attention on Cleveland in Hawaii and Dewey at Manila.

Considered apart from substance, Dobson's style is straightforward, readable, and sometimes witty; and he tries to make each chapter inviting by beginning it with a vignette, of which a few are pointed and apt. If this book had appeared as a paperback fifteen years ago, just after Walter LaFeber's *The New Empire* (1963), it might have served for contrast on an undergraduate reading list. Now, however, it must compete with Charles S. Campbell, *The Transformation of American Foreign Relations, 1865–1900* (1976), which is based on more penetrating scholarship than Dobson's and displays more mature judgment, in addition to a superb bibliography. To be sure, Dobson includes the Theodore Roosevelt and Taft administrations, as Campbell does not, but these chapters are the most superficial of all, not much more detailed than a textbook. All in all, Dobson might do well to cover less ground in his next book, cultivate the state department archives and the newspapers, and thus put his writing talents to better use.

DAVID M. PLETCHER
Indiana University, Bloomington

KEITH W. OLSON. *Biography of a Progressive: Franklin K. Lane, 1864–1921.* (Contributions in American History, number 78.) Westport, Conn.: Greenwood Press. 1979. Pp. x, 233. $17.95.

Keith W. Olson has written a useful study of a significant but largely forgotten progressive, Franklin K. Lane, whose career "paralleled the reform impulse" of his age (p. 3). Coming out of a devout Presbyterian family, Lane engaged in the late 1800s in good-government campaigns in California and worked as a muckraking journalist in New York City and Tacoma, Washington. Turning to law, he became a crusading city-county attorney in San Francisco and, in 1902, barely lost the California gubernatorial election. Although a Democrat, he received appointments from Theodore Roosevelt and Taft to the Interstate Commerce Commission, where, from 1906 to 1913, he was "the major force" (p. 45). Subsequently, as Woodrow Wilson's Secretary of the Interior, Lane played a substantial role in the development of federal policies regarding oil, waterpower, the Bureau of Indian Affairs, and Alaskan resources. He was, Olson argues persuasively, a man of integrity and principle, as well as an effective and thoughtful administrator.

Olson convincingly associates Lane with the mood and campaigns that fell under the rubric of progressivism. At one point, for example, he tells how Lane established for Interior employees a "Home Club"—a social organization offering a gymnasium, billiard room, and library—and held departmental songfests. From Lane's perspective, such contributions not only boosted morale but also demonstrated that the department was free of "caste line or snobbery" and that "people of moderate salaries" could achieve much through "cooperation" (p. 73). Here, indeed, was an illustration of the progressive quest for efficiency and moral community.

The book has notable strengths, especially in terms of research, organization, and a straight-forward writing style. Still, one wishes that it had a sharper interpretive edge. Although the introduction contains a convenient sketch of recent progressive historiography and suggests ways in which Lane's career does or does not "fit," Olson

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might nonetheless have made better use of secondary literature. In what ways, for instance, does the information concerning Lane's and Wilson's agreements and disagreements over diplomacy provide insights into progressive foreign policy—a topic that has received much scholarly attention? Olson's tendency is simply to stress the diversity and complexity of progressivism. Fair enough, but ultimately one wants to know more about the particular variety of reform for which Lane spoke. This is especially so because of Olson's belief that Lane's success "resulted from the extent to which his contemporary Americans shared [his] values and attitudes" (p. 175).

Olson achieves the first of his goals: showing how Lane's career "helps us understand what happened during those years." He is less successful in his second objective: using Lane's life to shed "light on what those years were all about" (p. 3).

LEROY ASHBY
Washington State University

GARLAND E. ALLEN. *Thomas Hunt Morgan: The Man and His Science*. Princeton: Princeton University Press. 1978. Pp. xvii, 447. $25.00.

Genetics is arguably the central discipline in modern biology, and no person more than T. H. Morgan saw the importance of that role or contributed so strikingly to its realization. At Columbia University (1904–28) and then at the California Institute of Technology (1928–45) Morgan played a decisive role in translating to the United States the explanatory ideals and research methods that were widely believed to account for the intellectual vitality and institutional success of science in imperial Germany. Morgan was an ardent investigator and a master teacher. His stimulus created two major schools of biological research, and from that in Pasadena emerged in significant measure our generation's most celebrated scientific enterprise, molecular biology.

Proceeding chronologically, Garland E. Allen attempts to relate Morgan's inquiries to the institutional, intellectual, and social framework within which the biologist's great endeavor was conducted. He is notably successful in establishing the first two of these relationships. We are offered a splendid portrayal of the famous "Fly Room" at Columbia where the Morgan group, working with *Drosophila*, created the basic generalization called the chromosome, then the gene theory of heredity. To this familiar story Allen joins a vivid and novel view of the deliberate importation of the new biology into California. Methodologically, Morgan was an impassioned experimentalist (Allen's discussion of this point provides one of the strongest features of his book); institutionally, he was capable of transforming that ideal into the concrete realities of laboratory, research program, and dedicated personnel. Allen makes a good case that it was Cal Tech's financial strength and its ambitious and informed scientific leaders, together with Morgan's vision of what biology must henceforth become, that fostered at that institution so many important elements of nascent molecular biology.

Obviously, Morgan's career was critical in the professionalization of biology in the United States. He defined problems and set standards and trained teachers for the coming generation. Morgan was also philosophically eager but naive. An unsophisticated materialism informed his numerous general writings and underlay his scientific work. Allen takes this matter quite seriously and, in fact, ventures upon materialistic interpretations, now of the social process, of his own. This approach is neither spelled out in detail nor made the essential fabric of the book. It lurks, however, at each critical turning, especially at those where institutional and economic concerns may be presumed to interact directly. The case of the California Institute of Technology (chapter nine) provides full exemplification of this intuition. As Allen makes no bold claims and gives few details, neither enthusiastic agreement with his insight nor peremptory rejection of his bias is in order. It must be stated, however, that because of the tentative and summary character of his discussion of this question, he has provided an analysis of the social setting of biology in twentieth-century America that, although suggestive, is less convincing than his excellent portrayal of science, scientists, and their institutions.

Within the scientific community ours are years of recollection and celebration of victories recently won; a growing literature reflects these concerns. Allen's portrait of a man and his science happily provides a more critical focus: he demonstrates the personal and institutional continuity joining organismic and molecular biology and thus offers an immensely rewarding view of the emergence of large-scale American science.

WILLIAM COLEMAN
University of Wisconsin, Madison

HARVARD SITKOFF. *A New Deal for Blacks: The Emergence of Civil Rights as a National Issue*. Volume 1, *The Depression Decade*. New York: Oxford University Press. 1978. Pp. xiii, 397. $14.95.

Harvard Sitkoff's *A New Deal for Blacks* is the first of a projected multivolume study of the development of the civil rights issue and the civil rights movement in twentieth-century American history. While it is the broadest account yet written of the major currents of black history during the 1930s, Sitkoff

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has not attempted to describe every aspect of black history during the decade. His account concentrates upon the developments that "ultimately led to significant radical reforms, but did not do so in the 1930's." The New Deal years, argues Sitkoff, were a turning point in the history of American race relations, a "watershed of developments whose outgrowth was a broad-based social movement aimed at bringing about a fuller participation of blacks in American society" (p. ix).

Within this context, Sitkoff describes changes in government, constitutional law, institutions, and the concept of race itself. Although the nation remained a substantially segregated society at the beginning of World War II, there were dramatic changes, particularly in the last half of the decade. As was the case in so many areas, Roosevelt and his administration more often responded to, rather than initiated, these changes, and Sitkoff subtly describes the ambiguous direction of the New Deal in this regard.

Because of his emphasis upon the development of the issue of civil rights, Sitkoff's material tends to focus upon developments in New York, where the major civil rights' organizations were headquartered, and Washington, where the dramatically expanded role of the federal government invited lobbying and agitation. There are two implicit themes in this portion of Sitkoff's study: the continuous bickering among civil rights organizations (and within the ranks of the groups), and the reluctance of most national political leaders—particularly Roosevelt—to take a firm stand on any issue that challenged the racial status quo.

These themes were related. While the inability of civil rights leaders to agree upon common tactics reflected disparate social outlooks and ideologies, it was also a product of one of the historic debates over political strategy. Should civil rights spokesmen demand full rights in the belief that compromise was ultimately self-defeating? Or should they accept limited gains in the hope that these would generate further pressure for change? The two major civil rights organizations of the 1930s aptly symbolized these approaches. In such celebrated cases as those of Angelo Herndon and the Scottsboro "boys," in persistent public demonstrations of racial egalitarianism, and in uncompromising attacks on every form of segregation, the Communist Party spurned compromise and insisted on laying the groundwork for revolutionary action. While the National Association for the Advancement of Colored People also used agitation and political pressure, it often emphasized conciliation and behind-the-scenes persuasion in an attempt to expand opportunities for blacks in New Deal agencies and programs.

The accomplishments of both approaches were decidedly mixed. The Communist Party failed to generate a genuine revolutionary impulse in the 1930s and, at times, seemed more intent upon destroying its ideological rivals than waging war against Jim Crow. At the same time, for all the NAACP's attempts to cajole and pressure the president and his administration to take a strong stand against racial discrimination, the basic structure of segregation remained unchallenged by the New Deal. It was to be a long way from President Franklin Roosevelt's timid commitment to equal rights, as expressed in Executive Order 8802, to the Civil Rights Act of 1964. In terms of change, however, it may have been an even longer way from that executive order backward to the dedication of the Lincoln Memorial in the mid-1920s, when blacks were roped off in a segregated section across the road from whites. Not the least of Sitkoff's many accomplishments is the distinction he makes between his own commitment to equal rights and his sensitive appreciation for the restraints under which individuals labored in the 1930s. Quoting one of Karl Marx's more insightful comments, Sitkoff notes: "Men make their own history, but they do not make it under circumstances chosen by themselves, but under circumstances directly encountered, given and transmitted from the past." (p. 329). With the first volume of this projected three-volume study, we have a clearer picture of just how difficult were the circumstances that generation faced.

DAN T. CARTER
Emory University

GRAHAM J. WHITE. *FDR and the Press.* Chicago: University of Chicago Press. 1979. Pp. xii, 186. $13.95.

Through a close examination of official transcripts and other, extensive primary sources, Graham J. White shows Franklin D. Roosevelt as a virtuoso in press conferences, capturing the admiration and support of reporters. Although Roosevelt was liked by journalists, he criticized the press and its owners for failing to report the news objectively, without added interpretation or analysis. Roosevelt believed that "85 percent" of the press opposed him, even in the face of proof to the contrary presented by his own staff. White further documents FDR's error by tabulating editorial positions on several Roosevelt actions and statements in the papers most reviewed by FDR himself and finding strong press support, even for controversial foreign policy decisions. White poses the problem: what were the sources of Roosevelt's almost unreasoning suspicion of, and hostility toward, the press?

The answer outlined is attractive but all too briefly explored. Rather than developing another "psychohistorical" attempt to penetrate a difficult character, White suggests a theory based on ideol-

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ogy. He shows that FDR viewed the American presidency as subject to alternations between Jeffersonian and Hamiltonian leadership. Viewing himself as another Jefferson, Roosevelt believed the press to be a tool of the oligarchy. His adherence to this formula, White contends, makes understandable his ambivalence toward the press; while Roosevelt respected the crucial role of the press in informing the people in a democratic state, he suspected that newspaper owners, as oligarchs, interfered with the free flow of facts from the president to the people. White closely analyzes Roosevelt's own review of Claude Bowers' *Jefferson and Hamilton* (1925), showing it to be a crucial stage in the development of Roosevelt's thinking. Although we have come to see FDR as brilliant in press conferences, we discover him bound to a political "theory" that was little more than a civics-class concept. The book joins other historical writing and memoirs of the period that tend to show that Roosevelt was both an adept in political skills and a *naif* in political ideology. White makes clear, however, that Roosevelt was not entirely *without* a political theory, as some have suggested.

The ideas provided in this short analysis are food for thought. While the hypothesis is not developed fully enough to be entirely convincing, it is logical and believable enough to attract attention as a bright, sharply focused depiction of one aspect of the Roosevelt enigma. FDR enjoyed reporters yet distrusted them; he praised and castigated them; he struggled against the press and usually emerged the winner. Perhaps this strange relationship, as White suggests, was rooted in the tension between his political brilliance and his ideological simplicity.

RODNEY CARLISLE
Rutgers University

CHARLES DEBENEDETTI. *Origins of the Modern American Peace Movement, 1915–1929.* (KTO Series in American History.) Millwood, N.Y.: KTO Press. 1978. Pp. xv, 281. $15.00.

Charles DeBenedetti skillfully reconstructs the history of the peace movement of the twenties in relation to diplomatic history, the expansion of United States economic power, and partisan politics. Although he firmly grounds his account in organizational history, he explores also the national and world views of citizen peace advocates that gave coherence to their diverse programs and give historical significance to their largely frustrated campaigns.

DeBenedetti gives conceptual form to his interpretation by a three-fold identification of peace advocates: internationalists oriented to problem solving who supported the United States' cooperation with the League of Nations; legalists oriented to international law who promoted, respectively, participation in the World Court or Outlawry of War; and social progressives oriented to an eclectic pacifism who endorsed various modifications of militant nationalism. These categories are not rigid. They are conceptual tools with which the author finds coherence in the shifting politics of coalition among peace advocates and between them and the public.

The narrative thread of this account is supplied by the efforts of peace advocates to organize themselves and to promote alternative plans of action. DeBenedetti recounts repeated frustration resulting from ideological and strategic differences and from the adroit manipulation of these rifts by partisan leaders, notably William E. Borah. Indeed, the peace advocates are portrayed as sometime pawns in the game of political advantage between, for example, Borah and Harding and Hughes or between the executive and legislature. In the process, the author concludes, foreign policy was accommodated to narrow, immediate economic and political interests.

The respective wings of peace advocates became defined in the process of organizing for action, although definition was obscured by the practical necessity of forming political coalitions. Liberal internationalists, for instance, discovered their faith in functional organization for the resolution of conflict as they organized in relation to the League of Nations issue. The instrumentality of their faith, the League, became a non-issue in American politics after 1923, a shibboleth for unacceptable modification of sovereignty, so that those whom DeBenedetti calls conservative internationalists, the advocates of international law, largely held the initiative. This wing promoted a brand of law that abjured binding overseas commitments, a version of Manifest Destiny that was idealized in the Outlawry of War program. Progressive pacifists had considerable influence, but they lacked a foreign policy instrumentality of their own. Their very eclecticism disposed them to relate outlawry to a more comprehensive program, and this situation enabled partisan politicians such as Borah to seize on outlawry as an alternative to participation in a World Court but to balk at any outlawry plan associated with a broader program. The emergence of the Kellogg-Briand Pact is interpreted in this context. It was a kind of backfire to initiatives from the French that were prompted initially by their own interest in a comprehensive American commitment.

Within this general account DeBenedetti offers suggestive insights, notably on the origins of collective security in early neutrality revision, on the

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maneuvering of Borah, and on the ways in which partisan politics reinforced the interests of an expanding American economy.

Despite repetitious analysis of the intellectual foundations of peace advocacy and perhaps because of occasional hyperbole, the author recounts the story with clarity and vigor. His interpretation is based on sound research in an impressive array of primary sources as well as on the relevant literature. It is well documented and is supplemented by a careful, informative bibliographical essay. It is a synthesis building on the work of scholars such as Denna F. Fleming, Selig Adler, Robert H. Ferrell, L. Ethan Ellis, William Appleman Williams, Joan Hoff Wilson, and others. A sound, coherent, and comprehensive analysis, this work relates the early modern American peace movement to the process of remaking foreign policy. It is an important contribution to diplomatic history of the United States in the twentieth century.

CHARLES CHATFIELD
Wittenberg University

MICHAEL SCHALLER. *The U.S. Crusade in China, 1938–1945.* New York: Columbia University Press. 1979. Pp. xiii, 364. $14.95.

Despite a record number of split infinitives, this is a lively and well-written explanation of what Herbert Feis called "The China Tangle." Most of the story is familiar, covered in detail by Feis, Barbara Tuchman, and a score of books and dissertations. Nonetheless, Michael Schaller provides new information and new insights. His interpretation is often forced, sometimes contradictory, and he lacks the command of the period or the literature for which one might hope, but this is his first book and a very promising start.

Most of Schaller's new material appears in two chapters he published as articles several years ago. The first of these describes ways in which the Roosevelt administration secretly waged war against Japan while the United States remained officially neutral. His case is persuasive: there were clandestine activities and more were planned. Schaller wonders archly if awareness of such plans, including the possibility of air raids against Japan from China, influenced Japan's decision for war with the United States but knows too much about Japanese policy to press the point. Still he seems to think historians have spent the last quarter of a century defending Roosevelt against Harry Elmer Barnes and Charles A. Beard.

The second parcel of new material appears in "SACO! The Counter-revolution in Action," a superb analysis of "how the actual behavior of Americans in China, as distinct from formal U.S. policy, induced the KMT [Kuomintang] to pursue a military solution and antagonized the CCP [Chinese Communist Party]" (p. 231). This is the story of Naval Group China, its commander, Milton Miles, and its appalling involvement with Tai Li's Blue Shirts in anti-Communist activities in the midst of the war against Japan. The chapter is marred only by a non sequitur in the last paragraph in which Schaller argues that much of Miles's activity was opposed by his superiors only because it was done "crudely and prematurely." Presumably he is implying here, as throughout the book, that the American government was eager to act against the Chinese Communists but was forced to delay and wished to be subtle because of the nuisance of having to fight against the Japanese. Are we so far removed from World War II that young historians really believe that American leaders were concerned primarily with the Communist menace from 1938 to 1945?

The early chapters of the book outline the origins of the American commitment to China. So much of the context in which American policy was made from 1938 to 1941 is missing that an undergraduate reading this section might be badly misled. Robert A. Divine's *Reluctant Belligerent* (1965) is probably the most accessible corrective.

The central chapters detail the rapidly deteriorating relations between Stilwell and Chiang, the role of Patrick J. Hurley, and the early contacts with the Chinese Communists at Yenan. An old story, but Schaller tells it well enough to read once more. He adds new material on Lauchlin Currie and some later thoughts of General George C. Marshall. The chapter on the Japanese surrender is good, demonstrating the desire of several American leaders to block Communist expansion in the months immediately following the war. His contention that they were willing to use force is not persuasive. He makes much of the fact that General Wedemeyer was asked to serve as Ambassador to China in the fall of 1945, seeing this as evidence that the Truman administration intended to continue Hurley's policies, but neglects to mention that the offer was withdrawn.

The last chapter is the weakest, as Schaller glances over the years 1946 to 1949. Twice he attributes to Dean Acheson's letter of transmittal for the "White Paper" of 1949 statements it did not contain. He fails to understand the purpose of Marshall's mission to China, is weak on the Chinese side, and distorts the events of 1949–50. Important correctives are available in the work of James Reardon-Anderson, Steven I. Levine, and in a collection of essays Dorothy Borg and Waldo Heinrichs will be publishing later this year.

Obviously, Schaller's work has serious flaws. It is,

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nonetheless, a good book by an able young scholar who will likely give us better as the years go by.

WARREN I. COHEN
Michigan State University

WILLIAM E. KINSELLA, JR. *Leadership in Isolation: FDR and the Origins of the Second World War.* Boston: G. K. Hall. 1978. Pp. vi, 282. $15.50.

William E. Kinsella's *Leadership in Isolation* clearly states in its introduction the thesis that President Roosevelt was "a defensive interventionist for he fully realized that the United States would be forced to enter an inevitable confrontation with the proponents of fascism" (p. 2). The author then goes on to support his thesis with an extensive review of the letters, reports, and talks that poured in upon the president and kept him fully informed of foreign affairs.

Turning back briefly to Roosevelt's duties during the First World War as assistant secretary of the Navy, Kinsella finds the future president's opinions of Europe shaped strongly by his 1918 tour of the Western front and the barbarisms of the Huns. The author then focuses upon the deepening crisis of the 1930s and Roosevelt's awareness that his nation must at some time be committed to battle to redress the balance of Europe. As the war rolled on towards Pearl Harbor, excellent summations are made of the mass of information that crossed Roosevelt's desk, and one sees as well his increased concern as a "leader in isolation." A "Conclusion" neatly summarizes the entire story and hammers hard upon the author's thesis.

One must have great respect for the author's industry and patience in pouring over the vast documentation that flooded Roosevelt's White House files, but it strikes this reviewer that Kinsella's thesis could have been strengthened if he had studied the currently available British and French documents that provide insights not found in the American archives.

For instance, President Roosevelt's intentions in giving his Quarantine Speech can be more fully understood from the Admiralty Papers. They provided this reviewer with initial evidence, hitherto buried in the U.S. Navy's archives, that the president in the fall of 1937 not only developed plans for a long-range naval blockade of Japan but also late in December sent Captain Ingersoll to London for discussions with the Admiralty about such a naval quarantine. Then, too, one gains a stronger image of "leadership in isolation" when one studies French and British prewar efforts to purchase American war planes, an effort that Roosevelt firmly supported time and again by personally intervening to assist in strengthening the European democracies.

One sees the same picture after Dunkirk and the fall of France when Churchill's government sought not only to obtain American arms for Britain but also, more important, to develop the American arsenal so the United States would have the means with which to battle Germany, Italy, and Japan. The president alway kept closely informed on this British search and not only facilitated it with Lend Lease, but also, when necessary, he directly expedited the search.

In the long record of events that led to Pearl Harbor, President Roosevelt provided "leadership in isolation," but it was a leadership that was even stronger than Kinsella reports.

JOHN MCVICKAR HAIGHT, JR.
Lehigh University

JAMES J. WEINGARTNER. *Crossroads of Death: The Story of the Malmédy Massacre and Trial.* Berkeley and Los Angeles: University of California Press. 1979. Pp. xii, 274. $14.95.

Recently Americans have been relearning the lesson of the insanity that is war. The movie *The Deerhunter* and the novel *Fields of Fire* forcefully dramatize this truth. Now on the scholarly level Southern Illinois University professor James J. Weingartner in his *Crossroads of Death* deals deftly with this strange irrationality. The author presents a detailed history of the 1944 killing of over seventy American prisoners by German Waffen SS troops at Malmédy as well as the trial and punishment of the accused.

Weingartner begins by analyzing the statistics of age, education, Nazi party loyalty, battlefield experience, and religious affiliation of the German troops at Malmédy and concludes that they were a "bewildering conglomeration of numerous subgroups of diverse character" (p. 5), neither predominantly sadistic nor humanitarian. Weingartner then recounts the facts of the massacre, the hunt for the alleged criminals, the prosecution of the defendants and the courts' decisions and sentences. He ends with a lengthy narrative of the trial's review through the army judicial chain of command, congressional committees, and the Supreme Court. The author describes the forces that finally canceled the imposed executions and brought the release of all of the convicted—problems with the evidence and the prosecution's methods, the attacks on the verdicts by American conservatives, such as the *Chicago Tribune* and Senator Joseph McCarthy, and the change in attitudes resulting from the Cold War.

The author explicitly states that the larger question he is considering is whether any military trial for war crimes held by an enemy can ever be just. His conclusion is no: war's nature is so irrational, the pressures on soldiers so intense, the considerations in battle so complicated, the natural bias, if

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not hatred, of the judging power so pervasive, that true justice is impossible.

This first full-length history of the massacre is a study of high quality and thoroughness. The author's particular strength is that he unites a sure historical grasp with a sharp probing legal mind so that his evaluations are careful and insightful. *Crossroads of Death* will satisfy almost anyone judging it on the most demanding standards of accuracy, but some may feel uneasy because at times the author has a tendency to adopt the role of defense attorney for the Nazi soldiers. Perhaps acceptance of his viewpoint depends on whether one places primary value on the liberals' concern for the legal protection of the innocent or the conservatives' desire that society properly punish the guilty.

WILLIAM J. BOSCH
Le Moyne College

ELMO RICHARDSON. *The Presidency of Dwight D. Eisenhower*. (American Presidency Series.) Lawrence: Regents Press of Kansas. 1979. Pp. x, 218. $12.00.

Volumes in the American Presidency Series are intended as comprehensive surveys of presidential administrations, grounded in secondary literature yet including "analysis and interpretation" (p. vii). Elmo Richardson's study is fairly successful on these terms. The author not only has used the available literature but also has made effective use of his own research at the Eisenhower Library. This research provides some of the best material in the book—for example, anecdotes illuminating Eisenhower's private personality and allusions to the president's "real" feelings on such issues as civil rights, as revealed in letters to friends.

Richardson demonstrates command of the relevant historiography, frequently noting the conflicting interpretations of various aspects of the Eisenhower presidency. Though he often offers interpretive comment, he does not always go into sufficient depth. One could cite several examples of this shortcoming. The question of Eisenhower's reactions to McCarthy and his followers, for instance, is only touched upon. Regarding the Eisenhower-Dulles relationship and the relative influence of each over American foreign policy, Richardson implies that Eisenhower was dominant (pp. 71–72) but offers little support for his view; his assertion that Eisenhower was not under Dulles's influence during the last year of the latter's life (p. 166), even if supportable, does not make the general case.

Richardson's interpretations are not always consistent. At one point he accepts the thesis that Eisenhower was an antimilitarist (p. 63), yet his evidence from the later years seems to support the opposite view. Indeed, certain of the author's overall assessments conflict with material presented elsewhere in the book. He concludes that Eisenhower was "a chief executive who did not even lead the federal government" and who relied mainly "on allies and partisans with slighter perceptions and less talent" (p. 195), yet at other points he implies that Eisenhower was his own man and praises many of his closest counselors—especially the "second team" of Cabinet officials after 1957 (p. 133).

On the other hand, Richardson offers some perceptive insights. Particularly useful is his suggestion that Eisenhower's reputed moderation was only "flexibility . . . fixed on a rigid base" (p. 195)—a more satisfying portrayal, given the evidence, than the common view that Eisenhower was a "modern," nondoctrinaire Republican.

In all, this is a worthwhile and interesting work. Thinner in analysis than one might hope, the book presents some fresh material and occasionally keen insight. Far too many factual errors mar the work, such as placing the Fifth Fleet off the coast of China; describing the Civil Rights Act of April 1960 as occurring after Kennedy's election; incorrectly dating Stalin's death, the Geneva Conference, and the Southern Manifesto; noting that Nelson Rockefeller ran for re-election in 1960; recording the vote for the Formosa Resolution as unanimous; and implying that the George substitute for the Bricker Amendment eventually passed. Such inaccuracies cannot be ignored in a synthetic account, but they do not negate its merits. Richardson's volume is a useful addition to the Eisenhower literature, relatively thorough and well grounded in primary sources. Clearly, however, it has not preempted the field from others who would offer interpretive studies of Eisenhower and his era.

GARY W. REICHARD
Ohio State University

ERNEST J. YANARELLA. *The Missile Defense Controversy: Strategy, Technology, and Politics, 1955–1972*. Lexington: University Press of Kentucky. 1977. Pp. xi, 236. $17.25.

Political scientist Ernest J. Yanarella has written a very useful history of the antiballistic missile controversy. He traces the ABM issue back to the Eisenhower years, beginning with the 1958 decision by Secretary of Defense Neil McElroy to give the Army authority to develop the Nike-Zeus, and then carries the debate through the next decade to its climax in the Senate votes for deployment in 1969 and 1970. The key figure is Robert McNamara, the Secretary of Defense under both Kennedy and Johnson. Yanarella is ambivalent in his treatment of McNamara, seeing him as both an effective critic of the ABM and its unwitting sponsor. On the one hand, McNamara fought against the antimissile system's supporters in both the Pentagon and Con-

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gress by arguing that the ABM was neither cost-effective nor reliable. Yet, at the same time, his organization of American research and development efforts gave impetus to what Ralph Lapp has described as the "technological imperative"—if a weapons is technically feasible, it will be produced. Under McNamara, American offensive and defensive weapons planners were placed in competition with each other, much like a football team on which the offense is pitted against the defense in practice. The sophisticated penetration techniques perfected by American research and development teams revealed deficiencies in the ability of the Nike-Zeus to discriminate between decoys and incoming missile warheads and thus led to the development in the 1960s of the far more effective Spartan-Sprint ABM system. Yanarella points out that the issue of Soviet capability was never examined; the ABM was designed to work against elaborate American penetration techniques, not against the presumably much more rudimentary Soviet methods. The result was that the ABM system developed sufficient technological merit to win support from the Joint Chiefs of Staff and from Congress and thus survive McNamara's formal opposition.

Yanarella is most successful in chronicling the various stages of ABM development. More than half his book deals with the period before 1967 and he shows that nearly all the points brought out in the later public debate simply recapitulated the internal struggle in the Pentagon under McElroy and McNamara. His attempt to remove the mystery from the technological imperative by demonstrating that it was a product of specific human decisions is less convincing. Throughout his study, Yanarella shows clearly that McNamara kept technical considerations under control by resisting both Army and congressional pressures for production of the ABM. In the last analysis, it was a political consideration, in the form of Lyndon Johnson's fear that the Republicans could exploit the ABM issue in the forthcoming presidential election, that led to the 1967 decision to deploy a light ABM system. The ultimate fate of the ABM also reflects the primacy of political motives, not irresistible technological pressures. Nixon used the bargaining chip argument, the notion that the United States had to have an ABM to trade off against the Russian system, to secure final Senate approval for deployment in 1970; he then proceeded to negotiate a firm limit on antimissile systems with the Soviets in SALT I. Contrary to Yanarella's emphasis on the dynamic role of technology, his study of the ABM issue suggests that rational politics still offers the best hope for controlling the nuclear arms race.

ROBERT A. DIVINE
University of Texas, Austin

KENT ANDERSON. *Television Fraud: The History and Implications of the Quiz Show Scandals.* (Contributions in American Studies, number 39.) Westport, Conn.: Greenwood Press. 1978. Pp. xii, 226. $18.95.

The $64,000 Question started a television trend in 1955. With a prize many times greater than its predecessors on radio, this quiz show caused a virtual national mania and attracted imitators throughout the world. The spectacle of contestants in separate glass booths added suspense, and even the losers got a Cadillac. The television quiz shows embraced the basic traits of the consumer culture and led to a gigantic national experiment in behaviorism, as producers manipulated both the contestants and their audiences. Twenty-two quiz and game shows were on the air by the end of the 1957–58 television season, with some revived today, other remembered only by trivia buffs, and the rest mercifully forgotten. *The Big Surprise, Twenty-One, The $64,000 Challenge, Do You Trust Your Wife? Dotto, Tic Tac Dough*, and similar programs featured contestants whose talents ranged from phenomenal memories to simple eccentricities. Although the programs displayed an outward appearance of legitimacy, many producers sacrificed honesty to suspense and showmanship. Albert Freedman, producer of *Twenty-One*, would defend his show as "entertainment and entertainment only" (p. 153).

The evolution of the quiz show mania is intriguing, but Ken Anderson rightly believes that it is more important to understand the public response to the scandals that erupted in 1958. Unfortunately, this is the book's weakest point. Anderson only partly succeeds in using the scandals as a litmus test of the American character. *Television Fraud* suffers from that academic condition of premature publication, and this slender little volume is conceptually sketchy and based solely on published sources. It does not really tell us anything new.

Yet the idea is a good one. "The most important elements in the quizzes," Anderson believes, "were not their exploitation of contestants and institutions but the materialistic tenets they presented" (p. 177). Producers and advertisers shaped images as well as people, and their formula remains unchanged. As recently as 1976, one game show producer frankly admitted: "We will be searching for the ideal contestant who I like to think of as the blue-collar guy with the white-collar mind" (p. 180).

When grand jury and congressional investigators began to probe the deception in 1958, editors and ministers followed their revelations with a barrage of criticism against the shows. Yet, public opinion polls revealed "a startling degree of national cynicism" toward the scandals, partly because only the contestants were punished (p. 181). The producers, advertisers, and networks escaped criminal action

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and deftly side-stepped government regulations. NBC dropped the beleaguered *Twenty-One* in October 1958, citing declining ratings as the reason, and CBS cancelled *The $64,000 Question* a month later, even though it had not yet been tainted by charges of fraud. The facade of innocence finally collapsed on November 2, 1959, when Charles Van Doren dramatically recanted his previous denials of complicity. Until his confession, Van Doren had played Alger Hiss to Herbert Stempel's version of Whittaker Chambers. The public response to Van Doren's confession was more sympathetic than condemnatory.

National character is an elusive concept, and Anderson's argument is intriguing if not always persuasive. Questions about public morality are especially difficult to answer, and these scandals should not simply be regarded as "noncriminal fraud without victims" (p. 178). Transcripts of *Twenty-One* and *Dotto*, which are included as appendixes, disarmingly reveal the essence of the fraud perpetrated on the American people. Although Anderson used congressional hearings, popular magazines, and newspapers to good effect, he conducted no interviews and cited very few books. Surprisingly, he ignored Erik Barnouw, *Tube of Plenty* (1975), Les Brown, *The Business Behind the Box* (1973), and Norman Blumenthal, *The T.V. Game Shows* (1975). To his credit, Anderson avoided muckraking, but his uncertainty about whether his book "is more a business history than a social history" (p. xi) remains a question this reviewer is unable to answer.

ECKARD V. TOY, JR.
University of Oregon

ATHAN THEOHARIS. *Spying on Americans: Political Surveillance from Hoover to the Huston Plan.* Philadelphia: Temple University Press. 1978. Pp. xiv, 331. $15.00.

This is a provocative effort by a hard-working New Left historian to chronicle almost a half century of abuses by federal intelligence agencies, principally the FBI, against American civil liberties. The author, a one-time consultant to the Senate Select Committee on Intelligence Activities, based the study upon a vast quantity of primary and secondary sources, including hundreds of documents recently brought to light by congressional committees and the Freedom of Information Act. There are large gaps in the story, of course, for many historical records remain closed and others have been destroyed. Still, Athan Theoharis has amassed enough evidence to demonstrate convincingly that internal security bureaucrats and their friends in the White House, in Congress, and in the media have frequently violated the law, ignored the Constitution, and become deeply involved in partisan politics.

The Central Intelligence Agency, the National Security Agency, and the Internal Revenue Service are among the agencies under scrutiny, but J. Edgar Hoover and the FBI are in the spotlight throughout the book. We continue to need a first-class biography of the awesome and dictatorial bureau director, who died in 1972, but, until that arrives, this study, along with the revealing recollections found in Ovid Demaris, *The Director, An Oral Biography of J. Edgar Hoover* (New York, 1975), will service scholars adequately. Among the book's strongest features is a detailed examination of Hoover's uses of presidential directives to expand bureau authority. Equally informative are accounts of the FBI's infamous COINTELPRO and COMINFIL programs and examples of the director's links to right-wing anti-Communists. Curiously, Theoharis overlooked Hoover's close relationship to Senator Joe McCarthy; in fact, the latter was virtually the former's protégé. But the focus of the book somewhat avoids the McCarthy era, perhaps in part because the author has discussed it in earlier books and articles. The stress is upon the immediate postwar years and on the 1960s and 70s.

The study is open to criticism for its poor organization, uninspired writing style, and above all for its strident left-wing bias. Theoharis assumes, for example, that the Cold War was an invention of ultraconservative, hysterical, and cynical Americans (predictably, Truman takes his lumps) and that the Communists in the United States were benign. He therefore can conclude, apparently, that all illegal attempts by federal officials to obtain information were unjustifiable. He argues that the United States has suffered a "breakdown" of its constitutional system and is currently experiencing Orwell's *1984*. Many historians will consider these and other interpretations controversial at best.

Nevertheless, *Spying on Americans* is a useful, up-to-date, solidly researched monograph. While it is unlikely to appeal to the general public or to those who eschew all polemics, it will be of value to scholars who continue to probe the secrets of recent American politics.

THOMAS C. REEVES
University of Wisconsin, Parkside

PETER W. DICKSON. *Kissinger and the Meaning of History.* Cambridge: Cambridge University Press. 1978. Pp. x, 197. $14.95.

A major purpose of the author is to discern a "connection between Kissinger's philosophical perspective on history and his role in formulating and executing foreign policy during a turbulent period in American history" (p. ix). Peter W. Dickson argues that in Kissinger's background, intellectual development, and public career one may find "a phil-

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osophical tradition and a style of diplomacy which together are predicated on the belief that the ultimate reality for man is neither nature nor the divine but history as an unending process" (p. 16).

Among the most important influences in Kissinger's life, according to Dickson, were the loss of thirteen Jewish relatives in the Holocaust of Nazi Germany and his relatively late intellectual awakening. The former encouraged a conviction that life was devoid of all purpose and history without meaning. The latter served to promote a sense of "the passage of time and the corresponding loss of freedom inherent in the aging process" (p. 45). The synthesis of these experiences, which led to a "deep concern for the preservation of human life and fascination with power politics" (p. 28), is to be found in Kissinger's undergraduate honors thesis at Harvard, "The Meaning of History" (1950), in which he evaluated the writings of Oswald Spengler, Arnold Toynbee, and Immanuel Kant.

Spengler's biological analogy of the historical process was rejected by Kissinger as too deterministic, as were Toynbee's efforts to find patterns in history on the basis of empirical research. But Kant's Protestant Idealism, with its idea of spiritual inwardness for the individual, was most appealing to Kissinger.

The author recognizes the complexities in evaluating the former Secretary of State's political record in the context of his indebtedness to Kant. In particular, Kissinger could not accept the German philosopher's faith in human progress and thus "was unable to find an ultimate value or purpose in this historical process" (p. 8). Instead, for Kissinger, the historical process involved the effort of man to "affirm his will and personality" (p. 7). Further, there has been an inner conflict between ethics and power in Kissinger that sets him apart from Kant, whose moral imperatives included the concept of a community of nations that would put an end to balance of power diplomacy and secret agreements between nations.

Dickson takes exception to the view of Kissinger as a cynical Machiavellian but notes Kissinger's tendency to view himself as an "American Bismarck" and concludes: "Realpolitik remains at the heart of Kissinger's political philosophy no matter the terms in which he may have chosen to express it before the American public" (p. 140).

As illustrations of this interpretation, the author discusses Kissinger's efforts in behalf of the Nixon administration to achieve detente with the Soviet Union, rapprochement with Communist China, and an equilibrium of power in the Middle East. These and other policies were related to Kissinger's belief that the United States can no longer expect to impose its will upon all areas of the world and must tailor its diplomatic goals to "a new international order based on the perception that diplomacy in the 1970's had become truly global" (p. 133).

Dickson's assessment of Kissinger's style and achievements is generally favorable but far from eulogistic. Similar to many of the statesman's critics, he faults certain assumptions and particular policies. He asserts, for example, that Kissinger seriously miscalculated the extent to which the Soviet Union would display restraint in regard to such matters as arms control, Berlin, and the Middle East. Nevertheless, he believes that Kissinger's positive contributions "will endure the test of time . . ." (p. 144).

Finally, Dickson's analysis of Kissinger's search for meaning in history adds a valuable dimension to any effort to understand some of the intellectual foundations which underlay his pursuit of peace and stability on a global scale.

THOMAS C. KENNEDY
University of Wyoming

I. M. DESTLER *et al. The Textile Wrangle: Conflict in Japanese-American Relations, 1969–1971.* Ithaca, N.Y.: Cornell University Press. 1979. Pp. 394. $19.50.

In the decade of instant or "pop" history books published by Normal Mailer or the *New York Times*, it should not be surprising that three scholars, two Japanese and one American, have produced a major monograph on a fairly recent Japanese-American economic conflict. Indeed we are fortunate that I. M. Destler (Carnegie Endowment), Haruhiro Fukui (University of California at Santa Barbara political science department) and Hideo Sato (Yale University political science department) have chosen to convey carefully the narrative of a trans-Pacific issue of the early Nixon years that might easily be otherwise lost in the shadow of changes in China policy.

The authors' approach is based on an arresting and useful assumption: foreign policy, they contend, follows from domestic concerns. Thus while this book is about international relations, it sets domestic governmental decisions within each country at the center of its stage. The subject matter is economic. "In 1969 the Nixon administration demanded that Japan enforce comprehensive controls on her sales of man-made fiber and wool textile products to the American market. The Japanese government declined to do so. Efforts to resolve the dispute included two summit conferences, two cabinet-level ministerial conferences, and at least nine other major negotiations. Together these dominated the economic relations of the two governments for the better part of three years" (p. 7). Yet the authors' conclusion is that the dispute was not an economic, but a political, one.

The concluding chapter is especially bold and

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challenging. A number of fundamental judgments are developed that require additional consideration and study. Included are generalizations such as: "the predominance of domestic economic issues"; "the impact of bureaucratic interests and processes"; and "the role of personality in history."

Additional merit lies in an almost encyclopedic compendium of information in text, chronology, and appendixes of major documents. Herein are all the sources collected for future reference on this subject.

Sadly, a weakness emerges from these strengths. The authors are so married to their subject that they lose sight of the context in which it must be set for proper understanding. Surely there are other examples of Japanese-American relations as well as other trade conflicts that should be mentioned here as background. The authors have interviewed many participants in the conflict as key sources, and while oral history provides a sense of immediacy, a perception of the place of this "wrangle" amongst others is missing.

So much useful information is here that the book will surely be valuable for some time. Probably, though, it will become a source work for others to write a more broadly cast history of the subject. This is a pity, since it is unlikely that future authors will be any more perceptive of the forces that drive foreign policy or of the ways in which these forces relate.

JERRY ISRAEL
Illinois Wesleyan University

CANADA

ARTHUR J. RAY and DONALD B. FREEMAN. *"Give Us Good Measure": An Economic Analysis of Relations between the Indians and the Hudson's Bay Company before 1763.* Toronto: University of Toronto Press. 1978. Pp. xvi, 298. Cloth $17.50, paper $4.95.

The authors' aim, in this frustratingly uneven book, is to discover what the so far untapped Hudson's Bay Company ledgers can tell us about the Indian-European, cross-cultural trade in the century 1670–1770. After a textbookish beginning, the book gathers sophistication and novelty toward the middle, culminating in an impressive and edifying quantitative analysis of data from the company's account books. One of the attractive features of this section is a graphic description of the company's post finances. Company factors were permitted by the London directors to exceed the official (fixed) exchange rate for goods by applying what became known as the "factor's standard" or "overplus"—a surcharge ranging anywhere from 10 to 50 percent over the official standard. Overplus was intended to cover out-of-pocket expenses, such as the cost of Indian gifts, incurred in the bayside trade. Presents to the Indians were fine so long as the total annual overplus was greater than the total annual expenses. Heads would roll when the difference between the two showed a deficit.

The remainder of the book (chaps. 15–17) flounders rather badly. The first two chapters of this trilogy attempt to explain Indian motivation for trade; the final chapter—a consideration of spatial concepts currently in vogue among economic geographers as these models might help explain the company's trade—is inconclusive, by the authors' own admission.

Things heat up in the two penultimate chapters, in which Arthur J. Ray and Donald B. Freeman seek to resolve whether the Indians who traded at the bay ("middlemen," they call them) had become commercial men in the Western economic sense of the term. They begin by contending that the nonmarket-oriented, traditional, Indian "economy" was in transition in the period 1670–1770; it now showed signs of market economy influence, specifically, the emergence of a distinctive middleman group of traders (Cree, Assiniboine, and Ojibwa) who brokered the trade between the company and the more interior tribes and bands—*middlemen* who were *price conscious*, were *sensitive to the costs* inherent in making the arduous journey to the bay, and who *advanced the price of goods* they bartered to interior bands and tribesmen. Such evidence, they feel, lays to rest the idea that Indian trading behavior was "non-economic."

The "market" labels—"middleman," "pricing," "mark-up," and so forth—are contextually malapropos; they are ethnocentric semanticisms that obscure, when they should be illuminating, the issue of Indian behavior and motivation. Consider the case of the so-called middlemen.

Cree-Assiniboine-Ojibwa hunters traded with or plundered their fur-rich neighbors for the same reason the Five Nations Iroquois did a century earlier: because their territories were stripped of marketable rodents by the mid-eighteenth century. The goods purchased at the bay were primarily intended for personal use and were only turned over, used, to fur-endowed neighbors when the urge to go shopping at the bay proved once again irresistible. As if in confirmation of this, Cree-Assiniboine-Ojibwa demand for trade goods and the quantity of furs they surrendered tended to be rigid, despite price fluctuations and company coercion. All of this the authors show. Such behavior can only be described as bizarre in "middlemen," which is precisely why "middleman" and all other such distortional "market" labels should be avoided in this context.

The post ledgers tells us much about Indian trade behavior—anomalous as it is—and a great deal about the dynamics of the exchange; they do not,

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however, reveal *why* Indian demand for European hardware and so forth remained inflexible; *why* the company could not entice the natives to surrender significantly greater quantities of furs; nor, quite frankly, what prompted these hunters to engage in this unholy enterprise or what significance and definitions these European commodities assumed in the Indian world. In short, Ray and Freeman have asked their computer printouts a question about Indian rationale they cannot answer—a question that is beyond the scope of the data base. The question is largely irrelevant to the thrust of the book, despite what the authors think.

CALVIN MARTIN
Rutgers College

DALE MIQUELON. *Dugard of Rouen: French Trade to Canada and the West Indies, 1729–1770.* Montreal: McGill-Queen's University Press. 1978. Pp. xi, 282. $21.95.

Fortunate indeed, as the author notes, is the historian with a relatively complete set of documentation on an eighteenth-century French merchant. Dale Miquelon's study of "Dugard of Rouen," based on sources in France and its colony, New France, is a significant contribution to the history of eighteenth-century trade, in general, and colonial trade, in particular.

In the introduction, an overview of conditions in France from 1715 to 1770 is briefly and adequately presented. Dugard's Rouen, physical and commercial, serves as a prelude to an able and skillful survey of the early career of the protagonist. In the chapters dealing with the formation of Dugard's company and the financing of his and his associates' enterprises, the complex and sometimes devious forms of capitalization are detailed. Liquidity, then as now, was and is a constant. The nature of merchandise sent to New France and the West Indies and the principal areas of interest of the Rouen merchant form the subject matter of the three following sections. A combination of elements, natural and human, led Dugard, as Miquelon indicates, to shift his economic interests in the later part of his career. He and his partners sought security in government-granted and subsidized monopolies. The many strengths of the work are undermined in the conclusion, "From Business History to the History of Society." It is either mistitled or fails in its quantum leap from the quantitative to the qualitative. In a series of appendixes, Miquelon presents interesting statistical tables that, however, are poorly integrated into the preceding narrative.

The bibliography is sound, although with omissions *qui chatouillent*. One would like to know if the Baby Collection was consulted at the Université de Montréal or the PAC. The transcripts of the latter are incomplete. The omission of Jean Tarrade's "Commerce colonial de la France" is surprising as are the omissions of Miquelon's predecessors and peers.

Notwithstanding these quibbling qualifications, Miquelon's work is an important addition to the ever increasing sophistication of Canadian historiography.

CAMERON NISH
Concordia University

ERNEST R. FORBES. *The Maritime Rights Movement, 1919–1927: A Study in Canadian Regionalism.* Montreal: McGill-Queen's University Press. 1979. Pp. x, 246.

Canadian regionalism is a much discussed but poorly understood phenomenon. This book is an important addition to our knowledge of Maritime regionalism in the twentieth century. It describes the Maritime Rights Movement between 1919 and 1927, showing how it surfaced amid the uncertainties of World War One and the optimism of the Progressive movement. Though initially most evident among farming and laboring groups, the Maritime Rights Movement ultimately found its most significant support among business and political leaders. In the twenties the movement turned to political action and attempted to form a regional bloc that could produce desired changes in federal government policy. When that tactic failed, in large part because Ottawa was more concerned about prairie regionalism, the movement attempted to gain its goals by using all the political power it could amass and by appealing to the rest of the country for "justice."

The causes of the movement's rise were clear. While some of the dissatisfaction stemmed from a general unhappiness over Confederation during the nineteenth century, most of it was the result of developments after 1900. Changes in freight rates particularly angered Maritimers, who increasingly found their industries unable to compete with the pampered companies of central Canada. Similarly, Maritimers found themselves, in the name of national growth, contributing through taxation to westward expansion even though they gained few benefits from it. Even the movement of prairie grains eastward tended to go through Portland, Maine—because of the interests of the railways in central Canada—and not through Maritime ports. These developments, and lesser ones (like the movement of the Intercolonial Railway's head office staff to central Canada when the Canadian National Railway was formed) indicated that the Maritimes were losing steadily within Confederation. Consequently, the movement manifested considerable

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activity and intense anger. Just as significantly, it was regional in scope, thereby transcending to some extent the localism and provincialism that had characterized earlier Maritimer outbursts.

By 1927 the movement had largely disintegrated, undercut by somewhat more prosperous times and the adroit maneuvering of federal politicians. Ernest R. Forbes particularly stresses the use Ottawa made of Royal Commissions and half-promises to placate Maritimers' demands. In short, Mackenzie King, preoccupied with stilling prairie discontent, decided to appear attentive but do little for the dissatisfied East. The ploy worked in the short run, but the Maritime Rights campaign did have an impact and left behind a regional consciousness that would contribute to subsequent feelings of unrest.

This reviewer has only one reservation about the book's approach. The Machiavellian way Ottawa manipulated the Maritime Rights agitation was certainly a major reason for the disintegration of the movement. One hesitates to accept, however, that the blandishments and manipulations of Ottawa could have been as crucial to the collapse of the movement as the structure and argument of the book suggests. The issues were important; the movement's arguments were persuasive; yet it achieved very little. Failure resulted, perhaps more than Forbes indicates, from the region's increasingly underdeveloped status in comparison with other regions. Thus, the Maritimes in the twenties produced rhetoric and exposed problems but could not generate the force to bring about change. There were too many traditional barriers to overcome, and there were too few networks provided by business, labor, farmers, and politicians. In that respect, the book indicates that an even more thorough analysis of Maritime society is needed before the nature of Maritime regionalism in the twenties (and afterward) can be fully understood.

Maritime Rights, then, is an important book, especially for a Canada caught in an anguishing period of self-analysis. It provides an important perspective on Maritime regionalism and shows how indifference in Ottawa fed regional dissatisfaction. Closely argued, informed by wide-ranging research, and enhanced by an engaging sytle, it is one of the most significant contributions to Canadian historiography in the last few years.

IAN MACPHERSON
University of Victoria

W. PETER WARD *White Canada Forever: Popular Attitudes and Public Policy toward Orientals in British Columbia.* Montreal: McGill-Queen's University Press. 1978. Pp. xii, 205. Cloth $15.00, paper $6.50.

This is a stimulating little book. Although the text and documentation are not likely to provide meat for specialists, the theoretical perspective and evidence mustered should prove of value to all those interested in comparative race relations. W. Peter Ward relates how persistent stereotypes about Asiatics produced a reservoir of ill feeling in British Columbia that sustained prejudicial activities from the late 1850s through the mid 1940s. Even as the socioeconomic composition of the frontier changed, a self-perpetuating racism continued to feed on new social, psychological, and economic tensions. Opportunistic politicians and journalists played a part, but almost every segment of the population expressed irrational fears of cultural contamination and competitive labor. At the core was an image of "non-assimilability." Egged on by Royal Commissions and virtually unchecked by sentiments of fair play, one movement after another attempted to deny resident Chinese, Asian Indians, and Japanese the full benefits of life in the province. Eventually near total exclusion of new immigrants was achieved. Unlike the United States, the second-generation was denied the franchise.

While the relationship between national attitudes and the unique circumstances of California is still debated by American historians, Ward convincingly demonstrates the powerful West Coast basis to Canadian racism. Presumably sharing many of the same prejudices, Ottawa permitted immigration long after the situation in British Columbia became explosive. Often the two governments seemed to work at cross purposes; at times the national leadership took the brunt of the hostility originally aimed at Orientals. Even with the aftershocks of Pearl Harbor, the Mackenzie King cabinet (perhaps to the nation's credit) equivocated for eleven weeks before ordering the evacuation of Japanese. Hence this volume offers yet another example of problems endemic to the federal arrangement.

The book's major argument, however, is that British Columbia was both pluralistic and segmented. With few points of day-to-day contact, polyculturalism only heightened the dominant white group's desire for homogeneity. There is nothing really original about this position—social scientists have long suspected that pluralism was the agar-agar for racism, but Ward skillfully uses the thesis to explain white supremacy: "Whiteness became the primary symbol of the homogeneous society which west coast residents hoped to create" (p. 22). Unfortunately, and in contrast to Alexander Saxton's California study, *The Indispensable Enemy*, the author never analyzes the potentially destructive cleavages brought from Europe that made a melting-pot myth and an element to exclude so necessary. Although, to be fair, Ward recognizes the complexity of racial antagonism, by overemphasizing the psychological and irrational aspects, he sometimes underestimates political and ec-

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onomic considerations. And, because he has discovered the specter of racism in all corners of the province, he sees little reason to explore the ways in which a scapegoat might serve otherwise competitive classes or interests.

In recent years, scholars on both sides of the border have experienced a catharsis: helping their nations purge the guilt of anti-Orientalism while reassessing shibboleths about pluralistic democratic societies. Perhaps unpleasant aspects of the past must be exorcised before one can move on to deeper analysis. Nevertheless, the very concept of racism—even when it does not attempt to affix blame—can impart blinders of its own. All societies have been guilty of prejudice. By reminding us of the sins of Canadian fathers, Ward's narrative may well provide historians in the United States with the perspective needed to push on to more fundamental issues. In this regard it is recommended reading.

MICHAEL R. GODLEY
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LATIN AMERICA

ALISTAIR HENNESSY. *The Frontier in Latin American History*. (Histories of the American Frontier.) Albuquerque: University of New Mexico Press. 1978. Pp. vii, 202. Cloth $12.50, paper $6.50.

This sprightly written series of six interpretive and descriptive essays plays on the theme of Latin American frontiers viewed from a comparative perspective. Alistair Hennessy argues, for example, that during the early period of conquest and colonization Spanish America developed along peninsular patterns—in towns rather than in unprotected rural settlements with a pastoral economy dominated by cattle raising. Because of the large Indian population and the migration to the New World of lusty *hidalgo* bachelors, the Spanish conquest was as much sexual as it was military. Spaniards also had to do more than simply tame a new environment as did English yeoman farmers with their women and families; the Iberians had to deal with a large indigenous population. To bring these Amerindians under control and to organize the economy, Spaniards introduced the *encomienda*, hacienda, plantation, and other peninsular institutions, archaic social and economic forms that lasted into the twentieth century. At the same time religious messianism and myths such as El Dorado, Seven Cities of Cíbola, Fountain of Youth, and the exotic Amazonians pervaded the Spanish conquest and colonial endeavor. In English America, in contrast, there were no myths, no minerals, no large Indian populations, and no self-justifying sanctions of crusading zeal. What socialized or held colonial frontiers together? In Hennessy's view the school, church, and nuclear family performed that function in English America; in Latin America it was the extended family, kinship ties and *compadrazgo*, and religious fiestas. Hennessy can delineate a myriad of frontiers—mission, Indian, maroon, mining, cattle, agricultural, rubber, Anglo-Hispanic, and political—that emerged as the Portuguese and Spanish colonies grew into nations. Very often crosscutting one another, these frontiers never developed in waves as in English America but in spots, archipelago style, with Brazil perhaps being the exception. If the North American wilderness was virtually monolithic and produced a self-reliant, independent, energetic, democratic settler carving a bright new future out of the virgin land, Latin American frontiers produced divergent prototypes such as the aristocratic land speculator, itinerant peddler, religious messiah, and cowboy bandit. Images of the latter—the gaucho of Argentina and the *bandeirante* of Brazil—have lingered longest perhaps, but their legacy, their mythic force, lay in the perpetuation of the Latin American frontier as violent, barbarous, and uncivilized.

On balance this is an excellent piece of comparative history, one of the best to appear since the publication of the fine collection of essays edited by Louis Hartz twenty years ago. Although Hennessy is at times discursive, a bit diffuse, and occasionally simplistic—the topic lends itself to that—he has a perceptive eye, an analytical, interpretive cast of mind, and cosmopolitan intellectual tastes and approaches, all reflected in his keen insights and thoughtful syntheses. American historians, in particular, will be well served by reading the essays on "The Turner Thesis and Latin America" and "Comparative Perspectives," both of which give broad dimensions not only to four centuries of Latin American development but also to that of the United States and the forces shaping its history.

JOHN J. TEPASKE
Duke University

ALLAN J. KUETHE. *Military Reform and Society in New Granada, 1773–1808*. (Latin American Monographs. Second Series, number 22.) Gainesville: University Presses of Florida, for Center for Latin American Studies. 1978. Pp. 234. $10.00.

The past year has seen the publication of three new studies on the Spanish colonial army and the defense system of the late Bourbon period: Leon Campbell for Peru, my own work on New Spain, and now the present volume on New Granada. These works offer different approaches, but each historian has had to test a number of themes and

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interpretations first developed by Lyle N. McAlister and María del Carmen Velázquez. We have examined the roots, or lack thereof, of praetorianism and sought to identify the relationships between the army and colonial society. Of the three researchers, Allan J. Kuethe has had the most difficult task. New Granada has not been subjected to the same number of detailed studies as New Spain or Peru, and the viceroyalty offered a remarkable diversity of populations, regions, and attitudes. Because New Granada was not often a priority region within the empire, administrative reforms such as the introduction of the intendancies did not take place.

Kuethe introduces his study by arguing that McAlister's conclusions about military privileges and praetorianism in New Spain cannot be extended to New Granada. Offering well-researched and detailed examples drawn from Colombian and Spanish archives, the author presents convincing evidence that the creoles in the interior provinces did not find much attraction in the army. Only in the coastal region around Cartagena did the *pardo* and Negro populations see militia service as a possible vehicle for social advancement. The Comunero Revolt in 1781 added a new ingredient in the interior since imperial officials foresaw the possibility of local militia forces turning their arms and training against Spain. Although Kuethe offers evidence that the existence of militia units prevented insurgent activity, the regime lost confidence in the old bonds of loyalty and obedience. Depending upon the attitudes of the viceroys and regional commanders, defense planning and military reform took place in what Kuethe aptly describes as a "piecemeal" fashion.

By adopting an essentially chronological approach, Kuethe offers an excellent overview of imperial, viceregal, and regional policies concerning the military. Unfortunately, there are some limitations in this method—particularly since the author wishes to penetrate beneath the senior administrative levels and to examine the place of the army in society. In an effort to be comprehensive, Kuethe jumps from province to province explaining different conditions and situations, but the result is sometimes confused and one dimensional. It is difficult to identify the forces at work within the regions and the attitudes of various sectors toward the militia and regular units. Since all of the privileged corporations were jealous of their own special *fueros* and prerogatives, it is not surprising that the militias became entangled in numerous jurisdictional disputes.

Kuethe's book underlines the importance of regional studies and the need for additional research before general conclusions can be made about Spanish America. That the Bourbon regime tolerated such wide diversity in its military structures both within New Granada and throughout the other viceroyalties demonstrates either flexibility or a lack of control. New Granada received band-aid treatment rather than the full reforming zeal of the late Bourbon period. The author adopts a traditional view of the impact of the Enlightenment and the growth of creole alienation. Perhaps further research into the incidents and conspiracies of the 1790s would confirm some contemporary opinions that they lacked real substance. Certainly, the strengths and weaknesses of the colonial army became most evident after the outbreak of the French Revolution. Kuethe has filled a lacuna in Spanish Bourbon military studies, and in a more general sense, his monograph should serve to focus attention on the viceroyalty of New Granada.

CHRISTON I. ARCHER
University of Calgary

ASUNCION LAVRIN, editor. *Latin American Women: Historical Perspectives.* (Contributions in Women's Studies, number 3.) Westport, Conn.: Greenwood Press. 1978. Pp. xiv, 343. $22.50.

During the past decade the revival of the feminist movement and an increased interest in social history have inspired a growing body of literature on the history of women in Europe and the United States. Yet historical scholarship on Latin American women has been relatively sparse, appearing until now as scattered articles, chapters, bibliographies, and edited documents. *Latin American Women* will be welcomed as the first collection of essays entirely devoted to the history of women in Latin America. In it eleven historians examine diverse topics for Mexico, Brazil, Peru, Argentina, and Gran Colombia from the Conquest until the early twentieth century. Through imaginative use of primary sources, they explore broad patterns of women's behavior, their contributions to society, and normative prescriptions of female roles. This important volume demonstrates that serious and exciting research is being carried out on Latin American women, but it also indicates how much remains to be done.

The first third of the book is devoted to two overviews of women in colonial Mexico and Brazil by Asunción Lavrin and A. J. R. Russell-Wood. These essays summarize much of what is presently known on these areas and provide new insights into the workings of colonial societies. By being overly ambitious, however, both authors at times tread on thin ground. Given the many lacunae in existing research, some generalizations are not fully substantiated, and occasional inaccuracies appear. (For instance, daughters were *not* automatically emancipated from the *patria potestas* at the age of twenty-five, as stated on page 30.) By trying to cover many topics in a limited space, several points are in-

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adequately developed, giving rise to some misleading impressions, such as that divorce as we know it today existed in colonial Brazil. Both essays generally present a static, abstract picture of colonial women.

The more modest case studies that follow are more successful. Perhaps the most outstanding, because it makes a provocative theoretical as well as descriptive contribution, is Elinor Burkett's article exploring the differential impact of the Spanish Conquest on Peruvian Indian men and women. Burkett challenges the standard vision of the double oppression of nonwhite women, in which racism is simply added to the burdens of sexism. She shows that in many ways Spanish sexism mitigated Spanish racism, placing Indian men, not women, at the bottom of the social hierarchy. Other essays focus on women of the noble Regla family, female convents, the independence wars, journal articles on women, and the feminist movement. An excellent chapter by Lavrin concludes the volume. It discusses broad changes in ideology and opportunities for women over four centuries, contrasts Latin America with Europe and the United States, and suggests thoughtful questions—accompanied by bibliography—for future investigations.

Taken together, the essays document long-term developments affecting women in Latin America. Particularly well established are significant changes in attitudes toward women's roles, a subject admirably treated in a number of articles. Other topics are not dealt with as consistently, frustrating attempts to compare different areas and historical periods. In this sense, the volume highlights the present limitations of the field. Although five essays cover Mexico and three Brazil, even comparisons between these two areas are premature because common problems have not been addressed in comparable ways. For instance, the juxtaposition of individual articles suggests that women entered nunneries more frequently in colonial Brazil than Mexico, but there is in fact no basis for such a conclusion. This phenomenon, like many others, must be quantified before comparisons can be drawn. Meaningful generalizations will require studying similar subjects in depth and establishing criteria for measuring attributes such as "passivity" and "independence," terms used throughout the volume without rigorous definition. Only then can women's position be tied into the context of specific political, social, economic, and demographic developments.

This collection of essays will be useful for undergraduates as well as specialists in women's history, and it should also prove valuable to a wider audience. For example, historians of reform movements and cultural dependency will want to read Hahner's, Little's, and Macías's articles, while those interested in colonial social structures and Indian values will find the pieces by Burkett and Gallagher illuminating. This major book enhances our understanding of colonial and nineteenth-century society and should stimulate further research in a new field that is gradually leading to a reassessment of Latin American history.

SILVIA M. ARROM
Yale University

D. A. BRADING. *Haciendas and Ranchos in the Mexican Bajío: León, 1700–1860.* (Cambridge Latin American Studies, number 32.) New York: Cambridge University Press. 1978. Pp. xvii, 258. $29.50.

David Brading has moved one step beyond previous studies of the hacienda in Mexico, offering an analysis of the development of a complete province over an extended period of time. One caveat, however; the bulk of his information on León deals with the colonial period so that the dates in the title induce false expectations.

The author already has published several important articles on this region. In the present work, the general chapter on the Bajío, topical chapters on population and the internal economic structure of the haciendas, and an interpretive chapter entitled "Agricultural Prices and Demographic Crises" include substantial sections taken verbatim (surprisingly without acknowledgement) from these previous publications. To complete the book, Brading has added new material on the structure of agricultural production in the province, chapters describing the fortunes and characteristics of the hacendado and ranchero classes, and an epilogue describing how agrarian reform affected land tenure in León between 1919 and 1940.

The results certainly are not a definitive work on León but rather a series of glimpses into, and speculation about, life in the province. Some of the author's analysis is most valuable. On the economic side, the discovery that hacendados in León increased profits in the eighteenth century by investing in capital improvements and by shifting from herding to the production of cereal grains confutes the general wisdom, which holds that hacendados increased profits by extension, not intensification of production. Further, the collective biographies of the hacendado and ranchero classes offer a number of new insights into the attitudes of creole and mestizo landowners. The material on the ranchero class is of particular value since this group, owners of small holdings, has been ignored in the previous literature.

On the other hand, much of the book's analysis is flawed and the interpretations rendered speculative by the fragmentary and special nature of the evidence available in public archives. Brading is forced

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to deal with some topics by reliance on examples that may or may not be typical and with others by making inferences from runs of data meant for other purposes, such as tithe records for agricultural production and baptismal, marriage, and burial records for demographic statistics. He recognizes the limitations and continually cautions the reader about the dangers inherent in his assumptions and his data. Overall, he manipulates the material in a very professional way.

Doubtless most experts will argue as I did with one or another of his assumptions and find some of his interpretations questionable, but as one who has sampled the type of documentation Brading used, I cannot bring myself to be overly harsh. This study of León is professionally done; it presents us with a mountain of information and with conclusions that at least offer benchmarks for others to surpass. The author has shown how such studies can be organized and carried out and hopefully will give courage to others to try to do likewise. For that he is to be praised.

JAMES D. RILEY
Catholic University of America

PETER H. SMITH. *Labyrinths of Power: Political Recruitment in Twentieth-Century Mexico*. Princeton: Princeton University Press. 1979. Pp. xvi, 384. Cloth $25.00, paper $9.75.

Students of Mexican politics and society have long debated the meaning and consequences of the revolution of 1910. Peter H. Smith has added to this discussion with an insightful analysis of political careers and elite composition in Mexico since 1900. In *Labyrinths of Power*, he sheds light on the changes and continuities in access to political power that have resulted from the revolution. He suggests that the revolution was important in creating and institutionalizing a system of elite circulation that promised greater political mobility to the same stratum of the population from which prerevolutionary elites were chosen. Political rather than structural changes were the essence of the impact of the revolution. Moreover, he suggests, political rather than structural solutions continue to be sought to problems confronting the country.

Smith has collected socioeconomic and career information on more than six thousand individuals who have held high-level office in Mexico in this century. The data are analyzed in terms of a useful distinction among prerevolutionary (1900–11), revolutionary (1917–40), and postrevolutionary (1946–71) elites. He finds that all three groups were overwhelmingly recruited from the relatively privileged middle class, although the revolution shifted power to different sectors within this class. But Smith also demonstrates that the patterns of political careers and elite circulation changed with historical periods. In the prerevolutionary period, access to the political elite was limited, careers tended to be stable—if not static—and turnover was minimal. During the revolutionary period, access to high office became more open, routes to success tended to be flexible and unpredictable, and turnover greatly accelerated. In the postrevolutionary era, access to the political elite remained open to a large number of people with middle-class backgrounds, career patterns became slightly more predictable than in the preceding period, and high levels of turnover among the elite became institutionalized. During this last period, Smith finds that careers became more professionalized, centralized, and bureaucratized, echoing the institutionalization of the political system itself. Characteristics of political careers are linked to the stable and authoritarian nature of the political system in a discussion of the "rules of the game" and an analysis of middle level political offices. Smith argues that the high rate of turnover is an enduring strength of the system and has been important in centralizing power and socializing new aspirants to elite status.

With a comparison of the careers of political and economic elites, *Labyrinths of Power* lends support to a growing body of opinion that the Mexican government, although certainly encouraging of capitalist economic development, is far from a tool of the capitalist class and seems to have "state interests" whose pursuit may bring it into conflict with the economic elites of the country. A final chapter on the political succession of 1976 is an excellent discussion of the maneuverings of the new president. Here, one wishes that Smith had been more expansive; the 1976 succession itself was unique in many ways and a broader consideration of its implication for the future of the system would have been welcome. Related to such a discussion, the author might also have provided a more extensive analysis of the nature of authoritarianism in Mexico, perhaps in place of the somewhat perfunctory consideration of elite theory presented at the outset of the book.

However, these are minor complaints. This book is admirable for its scope and scholarship and for the arduous and time-consuming process of data collection that it entailed. The data are consistently used with caution and an appreciation of their limitations, and statistical manipulations and resultant tables and figures are simply and clearly described. Smith has made a valuable contribution to understanding political power in twentieth-century Mexico, and scholars should be tantalized by this study to investigate further the connection between changes in elite composition and changes in public policy in Mexico.

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Wellesley College

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CHARLES D. AMERINGER. *Don Pepe: A Political Biography of José Figueres of Costa Rica.* Albuquerque: University of New Mexico Press. 1978. Pp. x, 324. $16.50.

From the days of Mary Wilhelmine Williams and the young Dana Gardner Munro, Central American historians have been accustomed to quality publications but never in abundance. Since 1976, however, the gleanings have turned rich, as evidenced by Woodward's *Central America* and the studies of the Somozas and Ubico by Millett and Grieb. Now it is our good fortune to add to the list Charles D. Ameringer's *Don Pepe.*

For the past three decades no Central American has been better known to the academic world than José Figueres Ferrer of Costa Rica. Accessible, articulate in two or more languages, considerate to students or newsmen alike, he once held the reputation of being Latin America's friendliest critic of the United States. (I still recall with pleasure a warm interview with Don Pepe at La Lucha in my graduate days nearly thirty years ago.) Figueres probably reached the peak of his standing in the U.S. in June 1958, when in the wake of the Caracas riots over a Vice President Richard Nixon visit, he came to Washington to explain that "you cannot spit on a foreign policy." Figueres has since had his lesser moments, and in the past few years his entanglement with international finance in general, and Robert Vesco in particular, has done him no good, despite their separate protestations of innocence.

Given Figueres's age and the suspicions that the Vesco affair have spawned, Ameringer's opportunity was unique. For the years before 1950 and after 1970 Ameringer has had to rely more on newspapers and secondary works, but between those dates he had unrestricted and presumably uncensored use of Figueres's complete private archive. No other historian can again have such access to the man and to his files. The author admits that even in his case some of the candor and openness of the whole research project began to disappear with Vesco's arrival in Costa Rica. The same holds for most of the many valuable interviews, for some of the key figures of the National Liberation era are already passing from the scene.

The book is subtitled *A Political Biography*, and this is important for the reader to accept. We learn little of Figueres's personal life, his family, or his divorce. We do learn a great deal of what he did, where he was during such and such an event, what he wrote and said. The book, in some ways an expansion of Ameringer's study of the Democratic Left, adds enormously to the published material on that and related topics; John P. Bell's *Crisis in Costa Rica* probably provides more analysis of the 1948 revolution, however.

Weaknesses in the book are few. The New Mexico Press should have insisted on more photographs; only the dust jacket has one of Don Pepe. Ameringer is a little unrelenting in the uniform shortness of his sentences and paragraphs, and sometimes the ablative absolute leads one astray. I would also have liked more Ameringer and less Don Pepe. But this is an important book about an exceptional figure and deserves the attention of any reader with the slightest interest in hemisphere affairs.

THOMAS L. KARNES
Arizona State University

ROBERT DEBS HEINL, JR. and NANCY GORDON HEINL. *Written in Blood: The Story of the Haitian People, 1492–1971.* Boston: Houghton Mifflin. 1978. Pp. xii, 785. $21.95.

The independent state of Haiti was born amid extraordinary bloodshed in an age in which violence was not only widespread but also the necessary midwife of national emergence. The debilitating peculiarity of the Haitian case is that, unlike the vast majority of instances of political change, the state never subsequently enjoyed peace or prosperity. The colonial violence against the slaves gave way to the violence against the upper classes. And from Toussaint L'Ouverture to François and Jean-Claude Duvalier violence has been the way to secure the presidency of Haiti and to maintain it. In 175 years only five peaceful transfers of office took place—and only four by popular election. Yet, despite this bloody aspect, Haiti has an interesting history that has not been well served by *Written in Blood.*

Robert Heinl and Nancy Heinl have chosen to focus on the politico-military competition for office. Their sometimes tedious narrative stresses the gory, sensational political minutiae taking place mainly in the presidential palace or the cities. The colonial period gets relatively short shrift—a mere 122 pages for the period from 1492 to 1803. But the rest of the book meticulously describes the seemingly ceaseless slaughters of the nineteenth and twentieth centuries. The best parts deal with the United States occupation between 1915 and 1934 and the presidency of François Duvalier (when one of the authors was commander of the United States Naval Mission to Haiti), reflecting both the heavy reliance on North American archival sources as well as personal involvement. The patient reader will find reference to every important political event and every person (and most factors) that affected political life: foreign relations with France, England, Germany, the United States, and the Dominican Republic; colonialism, and neocolonialism; race, class, and color; the poverty of state and people.

Written in Blood, however, suffers from two near

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fatal flaws. The first is a confusing, inelegant style where the surfeit of information tends to obscure the meaning of the changes described. The second is the inordinate reliance on the "story" at the expense of the historical evolution of state and people. The closest reference to social history is in the appendix on voodoo, and here the authors display gross ignorance and insensitivity to Haitian society and culture when they write, "Except for their chains, all that the slaves of St. Domingue could carry from Africa came in their hearts" (p. 669). The chronology and glossary are useful, but the bibliography lacks some vital references such as works by Philip Curtin, Thomas Ott, and Harry Hoetink—works that would have amply illustrated that a good and bloody story, however well told, does not necessarily constitute a good history. *Written in Blood* is simply not a good history. It may be consulted but it should never be emulated.

FRANKLIN W. KNIGHT
Johns Hopkins University

SHELDON B. LISS. *Diplomacy and Dependency: Venezuela, the United States, and the Americas*. Salisbury, N.C.: Documentary Publications. 1978. Pp. viii, 356. $16.95.

As the title suggests, this book is not a traditional bilateral diplomatic history. It is instead an attempt to analyze, within the framework of economic dependency, Venezuela's relations not only with the United States but also with the entire hemisphere. While this comprehensive study traces Venezuela's international relations from Independence to the mid-1970s, it emphasizes the post-1935 period, especially the last twenty years of democratic rule. Although he states that it was only after 1935 that Venezuela assumed an active role in inter-American affairs, Sheldon B. Liss nonetheless believes that some persisting principles have guided Venezuela's foreign policy throughout its history: adherence to international law, the peaceful resolution of conflicts, and the strengthening of the inter-American system.

Diplomacy and Dependency has much to offer the student of Venezuelan and hemispheric affairs. Although the work of a North American historian, it is written from the viewpoint of Caracas, not Washington. Its author, who correctly stresses continuity rather than change in Venezuelan foreign policy, takes a refreshingly nonpolemical approach to the wide variety of regimes that have formulated that policy. He provides fresh information or insight, moreover, on General Juan Vicente Gómez as a shrewd diplomatist, Nazi activities in Venezuela in the 1930s and 1940s, Venezuela's long-standing disputes with neighboring Colombia and Guyana (British Guiana), and the Betancourt Doctrine of nonrecognition of undemocratic regimes as a temporary departure from traditional policy.

The book, nevertheless, is disappointing in several respects. It suffers not only from careless organization, writing, and editing but also from factual errors as well. For example, General Cipriano Castro neither was illiterate nor did he lead his army into Caracas on May 23, 1899 (p. 27). General Eleazar López Contreras did not participate in a putsch on April 28, 1928 (p. 92), and President Raúl Leoni (1964–69) did not maintain a political alliance with COPEI, as the author implies (p. 180). The research, although extensive, is spotty. More use could have been made, for instance, of state department decimal files and Venezuelan newspapers. Furthermore, for a book that emphasizes Venezuela's dependence on the United States, it fails to take sufficiently into account the changing relationship between those two countries created by the increasingly nationalistic oil policies of López Contreras (1935–41), Isaías Medina Angarita (1941–45), and *Acción Democrática* (1945–48). Indeed, the historic petroleum reform law of 1943, the product of prolonged bilateral negotiation, receives virtually no mention at all. Finally, while much of the chronological narrative amounts to a conventional, politically oriented account of Venezuelan diplomacy, the two concluding chapters on the 1970s and on oil and OPEC set out an explicit *dependentista* indictment of the limits that North American dominance has imposed on Venezuela's freedom of action, politically and economically. The book would have been more cohesive had the dependency theme been integrated more smoothly and systematically into the text itself.

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CHARLES W. BERGQUIST. *Coffee and Conflict in Colombia, 1886–1910*. Durham, N.C.: Duke University Press. 1978. Pp. x, 277. $16.75.

Few episodes in Colombian history are as dramatic yet so confusing as the War of the Thousand Days (1899–1902) in which perhaps 100,000 people out of a population of 4,000,000 perished. An octogenarian president, a septuagenarian vice-president, revolts, coups, countercoups, railroads that went nowhere, conservatives dogmatically defending paper money, and exporters enraged over inflation give this period of Colombian history an exotic hue that Gabriel García Márquez immortalized in his novel *One Hundred Years of Solitude*.

In this his first book Charles W. Bergquist brings Colombian reality back down to earth in a lucid effort that will endear him to Colombianists for a

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long time. He solidly orders turn-of-the-century Colombian history around the interplay of economic forces and the rival ideological claims of the Colombian elite. Mastering the thinking of contending political groups—Nationalists, intransigent and Historical Conservatives, and peace and war Liberals—he effectively links much of their partisanship to their respective position within the Colombian economy. Coffee exporters, hacendados, merchants, importers, lawyers, and public employees tended to concentrate in only one or two of the political groups and thus responded partisanly when faced with economic and political change. The halting way in which the coffee export sector linked up with the international economy produced extreme fluctuations in the Colombian economy. Falling government revenues led to the printing of more and more paper money and the taxing of exports, including coffee. When economic contraction came in the 1890s, most groups blamed government economic policies. All sides felt threatened and scrambled for the political and economic position that seemingly best protected their interests. These rivalries grew in intensity and led to the Thousand Days War. At first it was played according to the gentlemanly rules of most nineteenth-century Colombian revolutions, but after seven months with no clear victor it degenerated into a guerrilla struggle that endangered elite control. Marauding anti- and pro-government forces carried out expropriations and summary executions that pushed the country to the edge of total social breakdown. With the growing realization that all could be lost, more rational spirits in the elite prevailed and moved the country toward peace and a new political and economic order. In the final denouement coffee enthroned bipartisan export-import interests and gave Colombia three decades of stability until *la violencia* (1946–58) exploded across the Colombian countryside.

For Colombianists Bergquist's book is a secure point of departure for the 1886–1910 period and a good counterpoint for studies seeking to explain Colombia's mid-twentieth-century period of violence or the elite-led civil wars of the nineteenth century. For Latin Americanists and historians of developing areas the book offers suggestive comparisons for other countries where ties with the world economic order have become the dominant economic force.

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BILLY JAYNES CHANDLER. *The Bandit King: Lampião of Brazil.* College Station: Texas A & M University Press. 1978. Pp. xii, 262. $15.00.

Billy Jaynes Chandler of Texas A & M University has given us a solid example of revisionist history in its soundest form. Myths may be for the masses, but scholars should strive to search for the truth. Fact, not folklore, should be the objective, and Chandler has succeeded in his quest. Basing his biography of Virgulino Ferreira da Silva (1897–1938)—Lampião—on archival research in Brazil and numerous personal interviews in the Northeast as well as contemporary newspaper accounts, the author has laid to rest the phantom of Lampião as a Brazilian Robin Hood.

Chandler places his subject within the broader theme of banditry in the Northeast, which erupted around 1900 and continued until the 1940s. These brigands or *cangaceiros* were originally enforcers who acted on the behalf of the landowners. Many, however, became brigands. But even as bandits they, like Lampião, continued to serve their wealthy protectors—*coiteros*—in suppressing the rural proletariat as well as preying on villages and farms that had not purchased protection.

The Bandit King is lavishly documented, perhaps too much so. In order to prove his thesis that Lampião was somewhat less than a vehicle of social protest, the author details every raid, robbery, skirmish, pillage, and pursuit for two decades. This close coverage, although trying, not only substantiates the theme but also clearly reveals the corruption of many of the local police and state officials. Chandler also touches upon incorporation of the Lampião legend into Brazilian folklore. Since the brigand and his band wandered relatively freely about the Northeast for years and he was interviewed by numerous reporters, his exploits captured the popular imagination. The image of a romantic revolutionary emerged and overshadowed reality. Shadow overcame substance and Lampião is now recalled in verse, story, and song—*Literatura de cordel*—as a freedom fighter. This tendency toward fantasy in folklore has been countered by Chandler's factual account, and for this he deserves our credit and praise.

LEWIS A. TAMBS
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JULIO HEISE GONZALEZ. *Años de formación y aprendizaje políticos, 1810–1833.* Santiago: Editorial Universitaria. 1978. Pp. 268.

Julio Heise Gonzalez convincingly maintains that the years 1810–29 in Chile should be viewed as a successful search by the creole leadership for a workable republican government after almost three centuries of Spanish rule. Because of the disorder so prevalent throughout this period, many historians have characterized the entire era as anarchic and compared it to the contemporary chaos in other

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Spanish American nations. Instead, Heise insists that disturbances necessarily typify the profound change from colony to independent nationhood, and he argues that these decades in Chile represented a constructive training period for the new governing class, a native elite numbering perhaps 150,000 men, women, and children. During these years this group of Chileans gradually overcame its political differences and, unlike elsewhere in Spanish America, established a stable governmental system by the 1330s.

Until the late 1820s the Chilean upper class was divided and confused in its political goals. The conservatives and the liberals, called *pelucones* and *pipiolos* respectively, championed such disparate notions as centralism, federalism, authoritarianism, parliamentarianism, and individual liberties. They belonged to loose factions whose membership often changed rapidly. Each grouping gained power and imposed its own constitutions on the divided citizenry.

The two most important heads of state in the 1820s were Bernardo O'Higgins, Supreme Director from 1821 to 1823, and Ramón Freire, who presided over several administrations between 1823 and 1827. Both of these leaders headed authoritarian governments. Unlike many other historians, Heise argues that O'Higgins and Freire were moderate liberals who never governed as dictators. The author notes that each carefully respected in theory and in practice an independent judiciary and took great care not to govern arbitrarily. Freire's resignation twice as chief executive and his refusal to suspend the Constitution of 1823, which he opposed, exemplify his respect for the rule of law. O'Higgins and Freire were realists who became convinced after independence that Chile would need a legal authoritarian system if continued disorder and dissension were to be avoided.

By the late 1820s, Heise demonstrates, Chilean aristocrats of both conservative and liberal tendencies were becoming aware of the disastrous consequences of unworkable political experiments and gradually closed ranks in support of a strong, centralized government. The failure of the liberal charter of 1823 was so apparent that even its chief architect, Juan Engaña, reluctantly concluded that such a document was unrealistic given Chile's recent colonial past. The ruling elite was so convinced by 1828 of the necessity of an authoritarian government that an overwhelming majority of the delegates to the constituent assembly that year rejected federalism and advocated a strong presidential system. The merchant and political organizer Diego Portales used this growing consensus among the upper class to establish a powerful, centralized government. A pragmatist rather than a theorist, Portales realized that Chile's colonial legacy of authoritarian tradition doomed to failure governments based on such notions as federalism, parliamentarianism, and respect for individual rights. The government he helped impose produced a generation of stability under three powerful executives from 1831 until 1861; the Constitution of 1833 incorporated Portales's presidential system and remained in force until 1925.

Heise's focus on the constructive nature of the state-making decades in Chile offers a valuable insight into why this nation alone among the Spanish American republics emerged in the 1830s with a workable governmental system. While stressing the importance of changing political attitudes after independence, the author also notes the contributions of economic, geographic, and racial factors in establishing the stability Chile experienced after 1829.

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ROBERT J. ALEXANDER. *The Tragedy of Chile.* (Contributions in Political Science, number 8.) Westport, Conn.: Greenwood Press. 1978. Pp. xiii, 509. $29.95.

It is now over six years since a military coup crushed Salvador Allende's Popular Unity government in Chile, bringing to an end, and then reversing, a decade of heady, and at times furious, social and political change. The questions raised by Allende's election in 1970 and the course of events that followed as his regime, with a little over a third of the popular vote, attempted to fashion a "Chilean road to socialism," were carefully watched and discussed not only by the current crop of fascinated academics but also by Italians, Frenchmen, and Spaniards who could see in Chile a scenario of their own political future. Just as nearly every tactic and move by Allende led to embittered wrangling not only within the bourgeois opposition but also especially among the finely tuned theoreticians of the left coalition itself, so also the postmortem accounts have been charged with ideological fervor. Demonology, however, now seems to be giving way to more reflective and balanced discussion.

Robert J. Alexander's new book is relentlessly fair-minded. He understands what the Christian Democrats were trying to do in 1964–70 and suggests that had they been re-elected for six more years their programs may have borne fruit. But he sees clearly their tactical blunders and unresolved factionalism. He finds the clumsy conspiracies of the CIA and the state department unconscionable but believes that, however ill-intentioned, they were probably not very important and certainly not decisive in overturning the Allende government. Indeed, in the final chapter, responsibility for the

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"tragedy of Chile" is handed out, in more or less equal portions, all along the political spectrum.

For Alexander, the "tragedy of Chile" consists of the destruction of what he calls a "uniquely open, tolerant, and civilized society." This came about because its "democratic and constitutional system" did not properly adjust to the pressures generated by rapid social and economic change (pp. 444, 52). But rather than placing his discussion of the events of 1964–77 in a framework of either modernization or dependency schemes, Alexander has concentrated on politics and attitudes. The result then is really a detailed and scrupulous political narrative informed through reading in the periodical literature of those years, personal interviews with participants, and the author's own wide experience in Latin America.

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SUSAN MIGDEN SOCOLOW. *The Merchants of Buenos Aires, 1778–1810: Family and Commerce.* (Cambridge Latin American Studies, number 30.) New York: Cambridge University Press. 1978. Pp. xv, 253. $22.50.

Susan Migden Socolow's analysis of the milieu of the wholesale merchant of colonial Buenos Aires is an important addition to the historical literature of the viceroyalty of the Río de la Plata. Following paths blazed by James Lockhart, Armando de Ramón, and David Brading, the author applies collective biographical techniques in her distillation of the extensive manuscript materials housed in the *Archivo General de la Nación* and the *Iglesia de la Merced.* The result is a convincing portrait of a group that enjoyed economic and social primacy during the closing decades of the eighteenth century.

The real strength of the study lies in its careful delineation of the wholesale merchants' origins, multiple roles, and place in society. Most of them hailed from northern Spain and the Basque region and retained an "overwhelming sense of European regional identification" (p. 112). Upon arrival in the colony the young Spaniard often married into an established merchant family, a pattern that assured group perpetuation. Sons of merchants in many instances chose not to follow in their fathers' footsteps but used their prestige and wealth to gain access to even more prestigious positions in the Church, military, or legal profession. That sons tended to move into other occupations reflected the high risks associated with wholesale merchant activity. Success was hard won and often proved ephemeral, seldom lasting beyond the second generation. European wars generally depressed trade in the viceroyalty, forced marginal wholesalers into bankruptcy, and helped to consolidate the position of the wealthiest traders. Despite the risks and uncertainty associated with the occupation, wholesale merchants exercised much local political power and enjoyed high social status.

Social status was particularly evident in the wholesale merchants' participation in lay religious institutions and colonial politics. Religious affiliation and activity not only was symbolic of a strong faith but also demonstrated public worth. Interestingly, many merchants held public office only as long as political activity was concomitant with social mobility. By the end of the eighteenth century the wealthiest merchants shunned public service because it was no longer perceived as an avenue to greater prestige. Socolow's prosopographical approach also reveals rivalries within the merchant community. Usually united in defense of commerce and special privileges, the merchant community divided over different concepts of trade, government policies, and personality conflicts.

Less satisfactory is Socolow's assertion that wholesale merchants controlled the economy of the countryside surrounding Buenos Aires and even that of more remote regions of the viceroyalty. Merchants *may* have "indirectly maintained the entire communications and transportation network of the region" (p. 54), but the statement is not sufficiently supported. And what was the point of view of the merchants of Tucumán, Mendoza, or even Lima? Their perceptions might well qualify some of Socolow's remarks about the nature of trade in the interior.

Finally, it is unfortunate that the author terminates her excellent study in 1810, for within twenty years wholesale merchants had broken with their almost exclusive urban orientation, invested in land, and constituted the nucleus of Argentina's emerging landed class. The transformation of the merchant, in my view, seems a more fitting conclusion than 1810, which is essentially a political watershed.

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University of Connecticut

GARY W. WYNIA. *Argentina in the Postwar Era: Politics and Economic Policy Making in a Divided Society.* Albuquerque: University of New Mexico Press. 1978. Pp. x, 289. Cloth $15.00, paper $6.95.

Gary W. Wynia is a political scientist whose chief concern is the theory and techniques of economic policy making, a subspecialty of games theory. He defines economic policy making as "adapting the capabilities of the state to the achievement of public purposes" (p. 4). His "principal objects of analysis," which he pursues single-mindedly, are "the tools or equipment used by political leaders to solve public problems" (p. 4). It is well to stress these points at

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the outset, for Wynia has written a political science treatise that often—in its structure as well as its vigorous narrative flow—bears a strong family resemblance to history. The resemblance, however, is mostly fortuitous, and the kinship remote.

On the narrow ground he has staked out for himself, the author is highly effective. He begins his narrative at the point in the 1930s at which the gravity of the world crisis forced the Justo administration to rescue the beef and grain industries (bulwarks of the export economy, power base of the landholding oligarchy) from free-market orthodoxy—in effect, the author implies, from themselves. In creating beef and grain boards and enacting regulatory measures, government economists annexed most of the decision-making capacity once controlled by the producers and traders; to the present day, they have never given it back. But industrialization was also beginning. Unwanted at first by government planners, it became a major preoccupation of the first Perón administration (1946–55); indeed, Peronist national planning used earnings from the traditional export sector to subsidize industry's capital costs as well as the creation of internal consumer demand. The bulk of Wynia's book consists of a commentary on the economic problems and policies of succeeding regimes: the stopgap military administrations of Aramburu and Levingston/Lanusse, General Onganía's inept aping of Brazilian technocracy, the disheveled limited democracies of Frondizi and Illia, Peronism *redux* (1973–76), the post-Peronist military. With the aid of excellently chosen statistics, Wynia identifies the competing interest groups and political constituencies in each emerging parallelogram of forces, and he judiciously weighs the competing demands of consumerism and capital aggregation, exports and domestic investment, nationalism and internationalism (as represented, for example, by the IMF). For Wynia (and not only him), the end of this trajectory was less satisfactory than the beginning: he clearly feels that the Peronist Social Contract of June 1973, a formal agreement among industry, labor, and government to achieve price stability and progressive income redistribution, was the logical solution that should have ended four decades of strife. Instead, of course, Argentina shortly thereafter collapsed into renewed economic disarray and unparalleled political savagery.

For historians the difficulty is that Wynia has abstracted his decision makers from their personal and social circumstances and has endowed them with a perfect grasp of economic doctrine, flawless sources of information, and Olympian impartiality and rationality. In effect Wynia has resurrected *homo economicus* and created for him a two-dimensional board game labeled "Argentina." This is positivism run amok: the contrast between the serene omniscience of Wynia's (hypothetical) political-economic elite and the gory shambles that Argentina has in fact become, would be uproariously funny if it were not so terribly painful. Historians will find the book useful as a reference tool, since the schematizations of economic crises are well conceived. But it is hardly a description of what the author bemusedly refers to as "the real world."

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

J. H. HEXTER. *On Historians: Reappraisals of Some of the Makers of Modern History.* Cambridge: Harvard University Press. 1979. Pp. viii, 310. $16.00.

Carl Becker and Historical Relativism. Wallace K. Ferguson and Hiram Hayden: The Renaissance Again—and Again. Fernand Braudel and the *Mode Braudellien.* . . . Lawrence Stone and the English Aristocracy. The Historical Method of Christopher Hill. Republic, Virtue, Liberty, and the Political Universe of J. G. A. Pocock.

J. H. HEXTER. *Reappraisals in History: New Views on History and Society in Early Modern Europe.* 2d ed. Chicago: University of Chicago Press. 1979. Pp. xix, 278. Paper $5.95.

This Historian and His Day. A New Framework for Social History. "Factors in Modern History." The Education of the Aristocracy in the Renaissance. The Myth of the Middle Class in Tudor England. Storm over the Gentry. Power, Parliament and Liberty in Early Stuart England. The Problem of the Presbyterian Independents. Puritanism—Roots and Branches. Personal Retrospect and Postscript.

CHARLES E. ROSENBERG, editor. *Healing and History: Essays for George Rosen.* New York: Science History Publications, and Dawson, Folkestone, Kent. 1979. Pp. viii, 262. $22.75.

CHARLES E. ROSENBERG, George Rosen and the Social History of Medicine. OWSEI TEMKIN, Medical Ethics and Honoraria in Late Antiquity. SUSAN KOSLOW, The Impact of Hugo van der Goes's Mental Illness and Late-Medieval Religious Attitude on the *Death of the Virgin.* SAUL JARCHO, The "Epidemia Rheumatica" Described by Lancisi (1711). DORA B. WEINER, Health and Mental Health in the Thought of Philippe Pinel: The Emergence of Psychiatry during the French Revolution. ERWIN H. ACKERKNECHT. German Jews, English Dissenters, French Protestants: Nineteenth-Century Pioneers of Modern Medicine and Science. W. F. BYNUM, Hospital Disease and Community: The London Fever Hospital, 1801–1850. CHARLES E. ROSENBERG, Florence Nightingale on Contagion: The Hospital as Moral Universe. LLOYD G. STEVENSON, "All according to the Constitooshun": Charles Dickens and Lead Poisoning. JACQUES M. QUEN, Isaac Ray: "The Greatest Amount of Good with the Smallest Amount of Harm." JOHN DUFFY, School Buildings and the Health of American School Children in The Nineteenth Century. GERT H. BRIEGER, Dyspepsia: The America Disease? Needs and Opportunities for Research. MICHAEL SHEPHERD, From Social Medicine to Social Psychiatry: The Achievement of Sir Aubrey Lewis. ARTHUR J. VISELTEAR, C.-E. A. Winslow: His Era and His Contribution to Medical Care. ALFRED YANKAUER, The American Journal of Public Health, 1957–1973. SAUL BENISON, Dr. George Rosen: An Appreciation.

DALE B. J. RANDALL, editor. *Medieval and Renaissance Studies: Proceedings of the Southeastern Institute of Medieval and Renaissance Studies, Summer, 1976.* (Medieval and Renaissance Series, number 8.) Durham, N.C.: Duke University Press. 1979. Pp. vii, 166. $14.75.

LEONARD E. BOYLE, Robert Grosseteste and the Pastoral Care. ROBERT S. LOPEZ, The Culture of the Medieval Merchant. EDMUND REISS, *Fin' Amors:* Its History and Meaning in Medieval Literature. MARCEL TETEL, Renaissance Aborted and Renascences: Scève's *Délie.* A. C. HAMILTON, On the Concept of the English Literary Renaissance. J. RICHARD JUDSON, Rubens and Moretus.

PETER CLEMOES *et al.*, editors. *Anglo-Saxon England.* Volume 7. New York: Cambridge University Press. 1978. Pp. x, 303. $37.50.

KENNETH HARRISON, Easter Cycles and the Equinox in the British Isles. PETER KITSON, Lapidary Traditions in Anglo Saxon England: Part 1, The Background; The Old English Lapidary. D. W. ROLLASON, Lists of Saints' Resting-Places in Anglo-Saxon England. NORMAN E. ELIASON, Beowulf, Wiglaf and the Wægmundings. CLAUDE SCHNEIDER, Cynewulf's Devaluation of Heroic Tradition in *Juli-*

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ana. N. F. BLAKE, The Genesis of *The Battle of Maldon*. RUTH WATERHOUSE. Affective Language, Especially Alliterating Qualifiers, in Ælfric's Life of St. Alban. RONALD E. BUCKALEW, Leland's Transcript of Ælfric's *Glossary*. SIMON KEYNES, An Interpretation of the *Pacx*, *Pax*, and *Paxs* Pennies. ALAN CARTER, The Anglo-Saxon Origins of Norwich: The Problems and Approaches. CECIL A. HEWETT, Anglo-Saxon Carpentry. P. R. ROBINSON, Self-Contained Units in Composite Manuscripts of the Anglo-Saxon Period. LINDA L. BROWNRIGG, Manuscripts Containing English Decoration 871–1066 Catalogued and Illustrated: A Review.

BERNARD E. BROWN, editor. *Eurocommunism and Eurosocialism: The Left Confronts Modernity*. (The Cyrco Press Series on World and National Issues.) New York: Cyrco Press. 1979. Pp. vii, 408, Cloth $18.95, paper $9.95.

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VIOLA HERMS DRATH, editor. *Germany in World Politics*. (Cyrco Press Series on World and National Issues.) New York: Cyrco Press. 1979. Pp. xii, 282. Cloth $15.95, paper $9.95.

GEORGE SCHWAB, The German State in Historical Perspective: An Introduction. NEILS HANSEN, U. S.-German Relations within the American-European Partnership. CATHERINE MCARDLE KELLEHER, Germany and the Alliance. PETER C. LUDZ, Foreign Policy in Germany: Ideological and Political Aspects of Intra-German Relations. JOHN STARRELS, The GDR's Relations with the Advanced Industrial Systems of the West. PETER HERMES, Economic Power and Political Responsibility. RALF DAHRENDORF, Europe and America: A Reassessment. WERNER STELTZER, International Aspects of the German Resistance Against Hitler. FREDERICK SPOTTS, The Churches and the Reconstruction of Germany. WENDELGARD VON STADEN, The Status of Women in a Changing Society. CHARLES R. FOSTER, Issues in German Education. ROBERT G. LIVINGSTON, The Marshall Plan Remembered. VIOLA HERMS DRATH, Commitment and Provocation—Return to Reality: An Assessment of the New Literature. HUGO MUELLER, Social Criticism in the German Novel of the Twentieth Century. JURGEN KALKBRENNER, Contemporary German Theater. BRUNO F. STEINBRUCKNER, Political Literature in Germany.

A. P. VAN GOUDOEVER, editor. *Romanian History, 1848–1918: Essays from the First Dutch-Romanian Colloquium of Historians, Utrecht, 1977*. (Historische Studies, number 36.) Groningen: Wolters-Noordhoff. 1979. Pp. 159. f 35.

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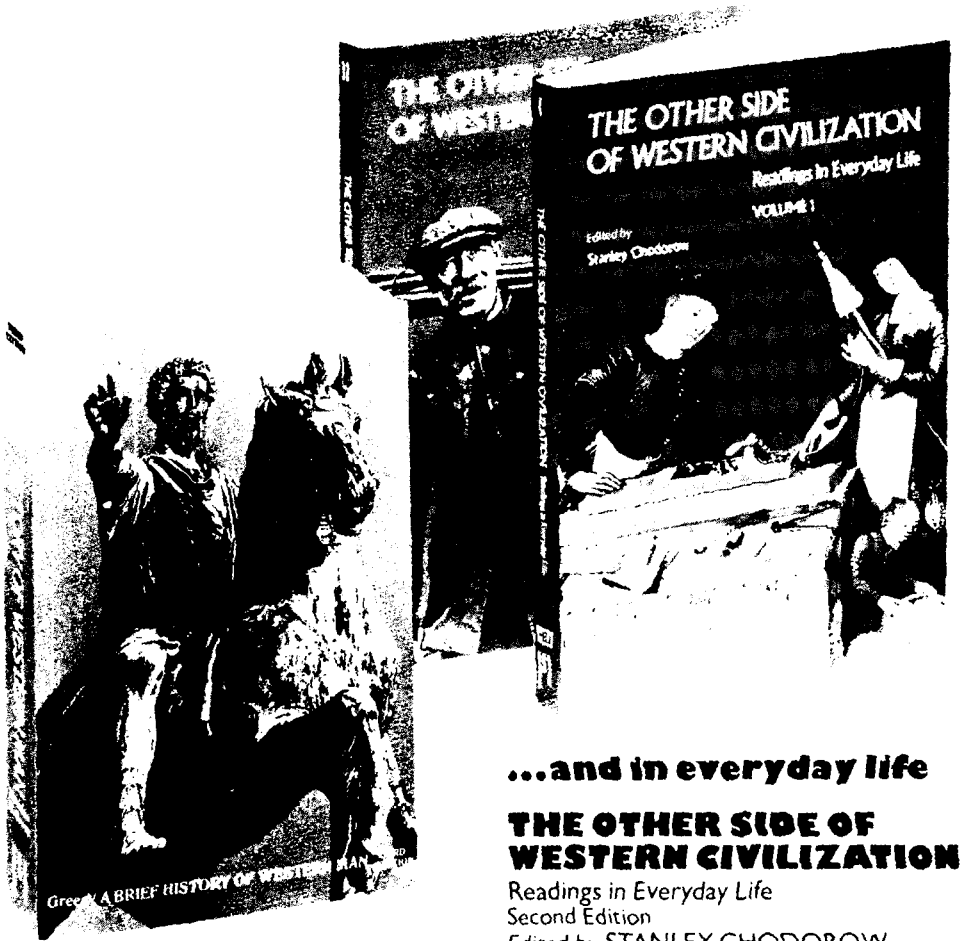
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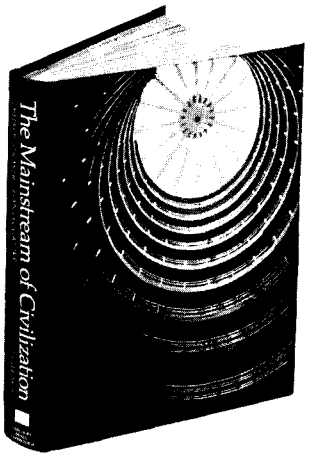
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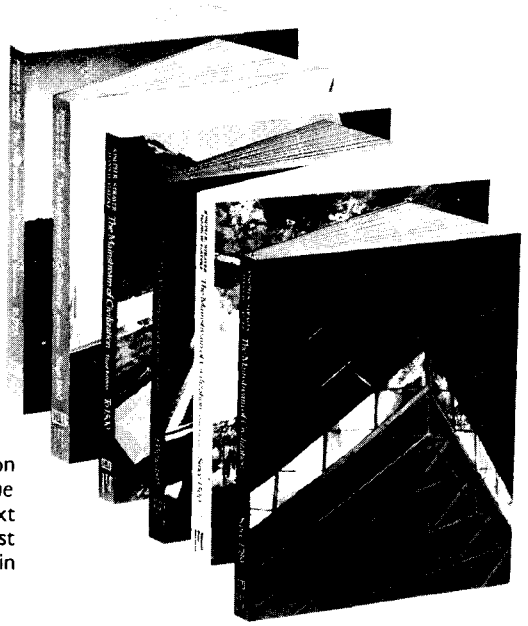
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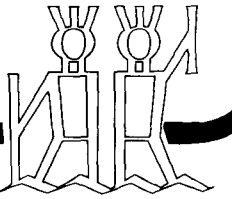
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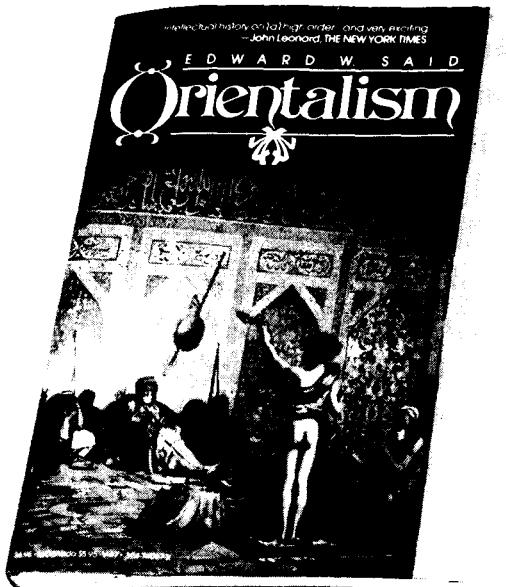
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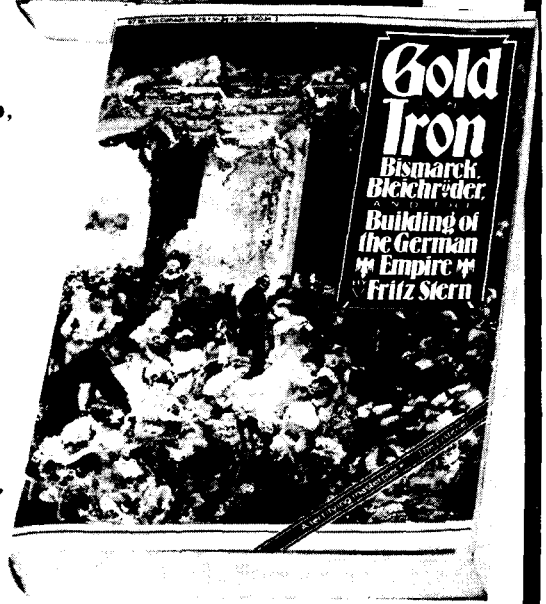


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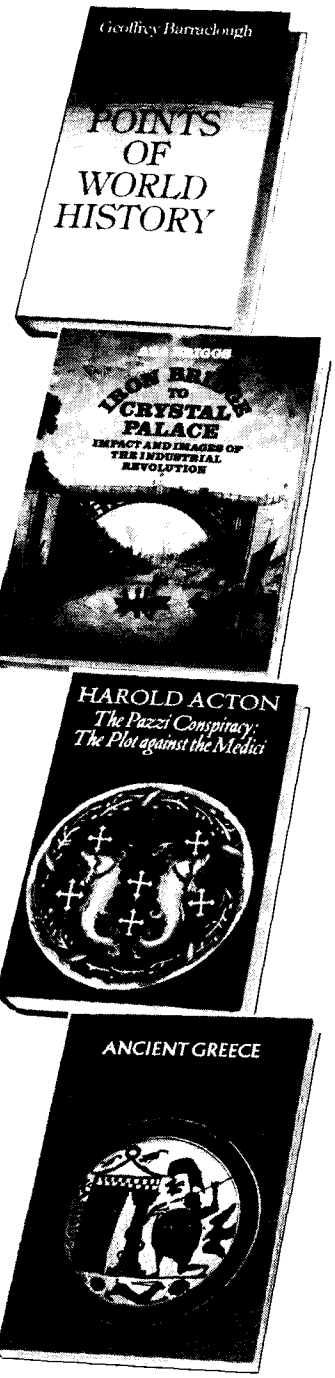
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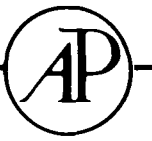
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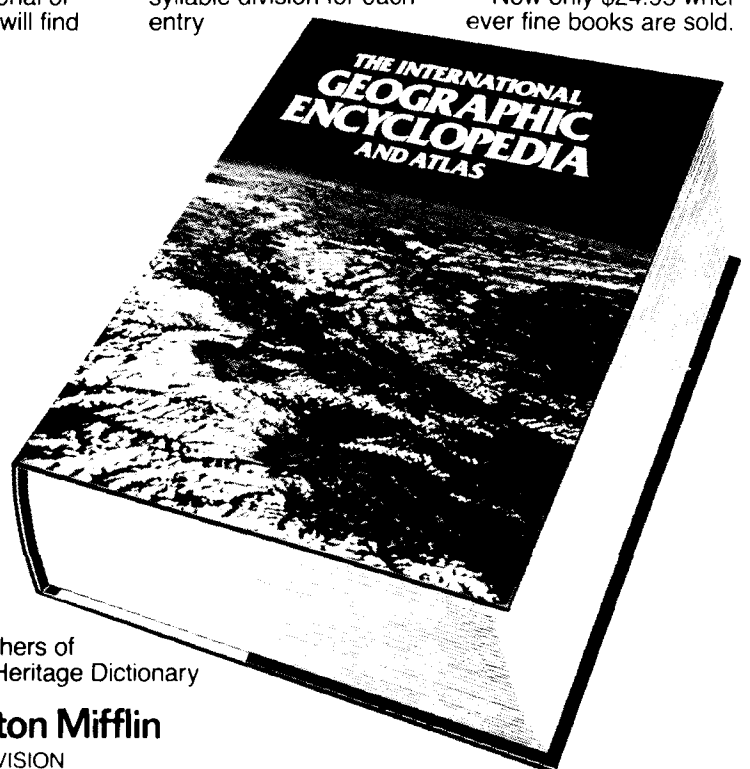
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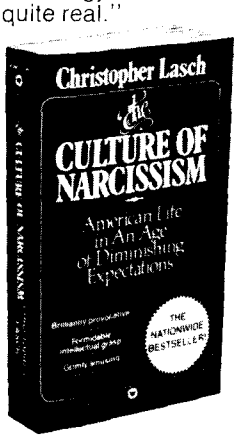
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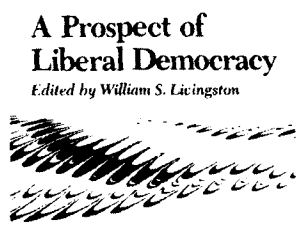


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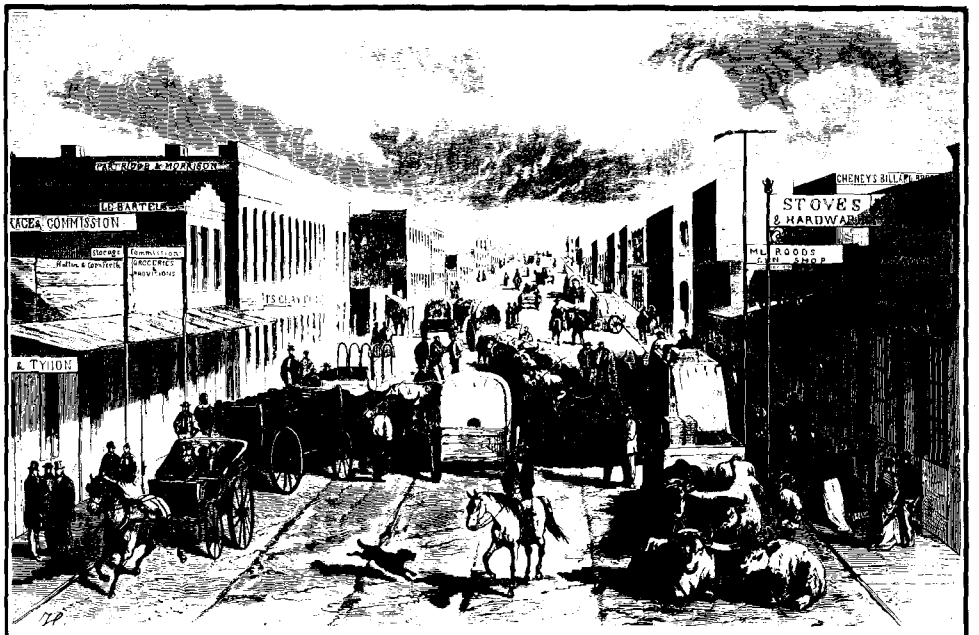
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